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XVII. *On the Rate of the Decrease of the Light given off by a Phosphorescent Surface.* *By* Lieut. L. DARWIN, R.E.*

I CARRIED out a series of experiments at Chatham with the view of determining the law of the rate of decrease of the light of a phosphorescent surface. The experiments were conducted by comparing the light given off by a surface covered with Balmain's luminous paint, with a sheet of tissue paper illuminated from the further side by a Sugg's burner regulated to give about the light of four standard candles. The surface coated with paint was one side of a thin metal vessel, which was filled with a mixture of ice and water ; the object of this was to keep the temperature as uniform as possible, as any increase of temperature increases the light given off by the paint. A sheet of tissue paper, of about the same size as the painted surface, was arranged just above it so that the light of the burner illuminated the tissue paper from the further side to the observer. The whole was enclosed in a box with an opening at one side, through which the light of the burner reached the tissue paper only, and opposite to it a small hole through which the observations were made. In this way only the light from the two surfaces reached the eye, and the light of the room did not reach the

* Read December 16, 1880.

surface of paint. Besides these reasons for observing through a small hole, it is better to do so, because the light given off by the illuminated tissue seems to vary with the angle at which it is observed, and the readings would not be constant if the eye were not in a fixed position. Experiment showed that the diffused light in the box from the illuminated tissue paper was not enough to have any effect on the phosphorescent paint. Both the paint and the tissue were observed through a solution of cupric sulphate, and blue glass; this did not, as far as I could see, cause any alteration in the colour of the paint. If the blue glass alone was used, the tissue looked pink in comparison with the paint; and with the cupric sulphate solution alone it looked green; but with the two the colour was so nearly imitated, that when the intensity of the light was the same I could only distinguish the two surfaces by their positions.

The illumination of the tissue was estimated by the distance of the burner from the tissue; this assumes that the light given off by the tissue varies directly as the light striking it on the reverse side. Bright sunlight was reflected onto the painted surface by a mirror; and after it had acted for a few seconds, the room was darkened, with the exception of the light of the standard burner. For five minutes before this time, I took the precaution of hiding my eyes, so that no light could get at them. As soon after this as possible, observations were taken. Two slightly different methods were employed. At first the burner was placed at a definite distance from the tissue, and the time was noted at which the paint and tissue first appeared to be equally bright, and also that at which they first appeared to differ again in illumination; the mean of these two times was taken as the time of equal illumination. The burner was then moved away to a further fixed position, and another observation taken in the same manner. After five observations had been taken in this way, the rate of decrease of the light became very slow, and this method did not work well. The distance of the burner to obtain equal illumination was then found by moving it backwards and forwards, and noting the position at which the illumination of the two surfaces appeared the same; this was done four times as rapidly as possible, two before and two after the time at which an

observation was wanted. The following table gives the mean of two series taken in this manner. The first column gives the illumination of the paint: the light given off by the tissue when the burner was nine inches from it is taken arbitrarily as 100; and the other illuminations are calculated accordingly from the distance of the burner. No absolute standard was attempted, as the light of the tissue would vary with its thickness, probably with the angle at which it was observed, and with the exactness with which the colour of the paint was imitated by means of the glass and solution. The second column gives the time at which the observation was taken.

TABLE.

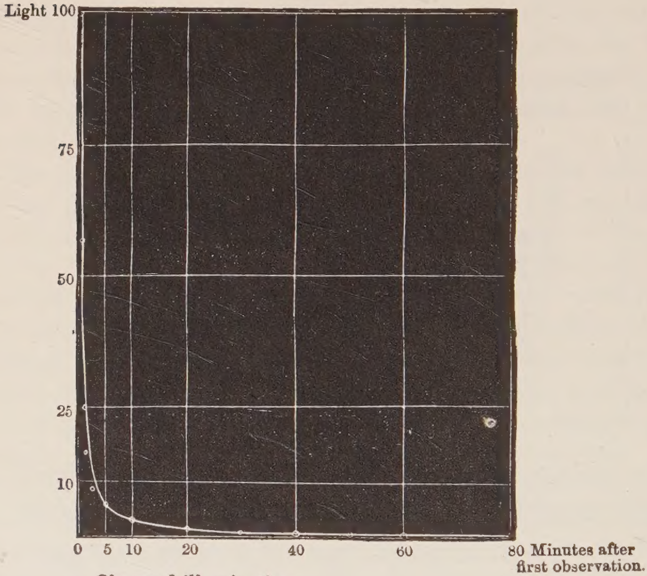
Observed Light.	Observed Time.		Calculated Time.	
	m.	s.	m.	s.
100·	0	0	0	0*
56·5		18		19
25·1		45	1	8
14·1	1	33	2	11
9·04	2	34	3	25
6·10	5	0	5	0*
2·89	10	0	9	59
1·34	20	0	19	53
·782	30	0	31	52
·604	40	0	39	41
·433	50	0	53	0
·376	60	0	60	0*
·285	80	0	76	14

I also made an attempt to calculate a curve which should as nearly as possible coincide with these results. The form of the curve which naturally suggested itself was $\frac{A}{t+B}=l^c$; t=time, l=light, A, B, and C being constants. A curve of this form to pass through three points (*) was calculated: A=26·1, B=·498 minute, C=·86. That is to say, the rate of decrease of the light varies as the light to the power of 1·86; and at that rate the light would have been infinitely great 30 seconds before the first observation. Taking the illuminations as obtained from the observations, the time of the observations was calculated according to the above formula; and the result is given in the third column. Three of the observed times differ from the calculated times

more than can be accounted for by errors of observation; and it appears that the equation does not give quite the correct curve. From this and from another series of observations, it appears that in its lower parts the curve is very nearly of the form $\frac{A}{t+B}=l$; that is to say, that the rate of decrease of the light varies as the square of the light.

By comparing several series of observations it appears to me that the rate of decrease is quite independent of the intensity of the first illumination.

Diagram showing the Rate of Decrease of the Light given off by Phosphorescent Paint.



o Observed illumination.

—— Curve calculated from the equation $y^{\cdot 86}=\frac{26\cdot 1}{x+\cdot 498}$.

XVIII. *Notes on the Construction of the Photophone.* *By* Professor Silvanus P. Thompson*.

(1) In the selenium photophone, light of varying intensity is received upon a prepared surface of sensitive crystalline selenium, the electric resistance of which it thereby changes.

* Read January 22, 1881.

In the construction of the receiving "cell" it is obvious that certain relations must hold between the dimensions of the sensitive surface and the degree in which a given quantity of light will change the electric resistance—relations which ought to be observed in the construction of the instrument, and which are certainly worthy of investigation.

Professor Bell's typical selenium-cell consists of a small cylinder about 2 inches in diameter and $2\frac{1}{2}$ inches in length (giving at most a superficial area of 15·8 square inches available), built up of alternate disks of brass and mica, filled between the edges of the brass disks with selenium, and having alternate brass disks connected up in multiple arc. This cell, in his usual apparatus, is placed at the bottom of a parabolic mirror.

Certain experimental observations made in attempting to repeat Prof. Bell's experiments led the writer to query whether this arrangement was the best possible one, and suggested an investigation, of which the following paragraphs are the chief points.

(2) THEOREM I.—*With a given maximum of incident light distributed uniformly over the surface, the change of electric resistance in a selenium-cell will vary proportionally with its linear dimensions, provided its parts be arranged so that on whatever scale constructed the normal resistance shall remain the same.*

Suppose there to be a cell of a certain size, having a certain normal resistance (*i. e.* a certain resistance in the dark as measured under a standard electromotive force), and presenting a certain area of surface; then, if a perfectly similar cell be made on a scale n times as great (in linear measure each way), the same total amount of light falling upon its surface will produce n times as great a variation in the electric resistance.

The proof of this theorem depends upon the law discovered by Professor W. G. Adams *—namely, that *the change in the resistance of selenium is directly as the square root of the illuminating-power.*

For let it be supposed (as in the proviso of the theorem, introduced so as not to complicate the electrical conditions) that the enlargement should be to the scale $n:1$ in all respects, save only in the depth of the selenium film, the brass con-

* Proc. Roy. Soc. vol. xxv. 1876, p. 113.

ductors being the same in number as before, but of n times their former size, touching selenium along edges n times as long as before, the intervening selenium films being n times as broad as before. Such an enlargement will leave the normal electric resistance where it was before, provided the depth of the selenium films be not increased—though, as the photo-electric action is almost entirely a surface action, a slight increase in the depth of the film would probably produce no great change in its electric sensitiveness.

Suppose the light to be caused, by appropriate optic means, to fall upon the whole enlarged surface uniformly. The linear dimensions being increased in the ratio $n : 1$, the area will be increased as $n^2 : 1$. The average intensity of the illumination will now be $\frac{1}{n^2}$ of what it was. Each portion of surface equal to the original surface will receive but $\frac{1}{n^2}$ part of the whole light.

But by Adams's law the change of electric resistance is proportional to the square root of the illumination. Hence the electric effect over each portion of surface equal to the original surface will be $\frac{1}{n}$ of the original electric effect; and, since the effect is proportional also to the amount of surface which is under illumination, this quantity $\frac{1}{n}$ multiplied into the ratio of the enlarged surface to the former surface ($n^2 : 1$), gives for the total electric sensitiveness of the enlarged cell a value n times as great as that of the original cell. Thus the proposition is proved.

(3) THEOREM II.—*With a given maximum of incident light the change of electric resistance will vary in proportion to the third power of the linear dimensions of the cell, if, while its linear dimensions are increased, the absolute thicknesses of the brass conductors and of the selenium films remain the same as before, and their number be proportionately increased.*

It was supposed above that the surface was increased n times by an enlargement in length and breadth, which left the total normal resistance where it was before. But since the breadth of the films is dictated solely by practical considerations of construction, the increase of linear width to n times will en-

able n times as many conductors to be employed; and the thickness of the selenium film may be reduced to $\frac{1}{n}$ of what it was reckoned above. This will reduce the total normal resistance of the cell to $\frac{1}{n^2}$ of what it was reckoned above, and would therefore make it n^2 times as sensitive were its resistance the only one in the circuit.

Combining this result with the former, we obtain the result that the change of electric resistance exhibited by the cell of linear dimensions n, under the influence of a given quantity of light distributed uniformly over its surface, will be n^3 times as great as that exhibited by a cell of linear dimensions 1, provided that the absolute thickness of the films and conductors remain the same (the resistance of the brass conductors themselves being reckoned small).

(4) The practical inference from this is, that the selenium-cells should be made as large as possible, and that the beam of light received by the mirror from the distant station should be so constructed as not to concentrate the light on one point of the selenium, but to distribute it uniformly over the sensitive surface.

Now the supposed advantage of the parabolic mirrors hitherto employed is that they collect parallel rays to one focus. If this be no longer necessary or advisable, then some other form of mirror than that of the paraboloid of revolution ought to be employed.

(5) A short cone, polished on the interior surface, appears therefore to offer certain advantages over the paraboloid in respect of its distribution of light, besides being far cheaper to construct. It only remains to calculate the appropriate angle of aperture that shall, with a cylindrical selenium-cell of given length, give the greatest available linear aperture and reflect into the cell the greatest number of effective rays.

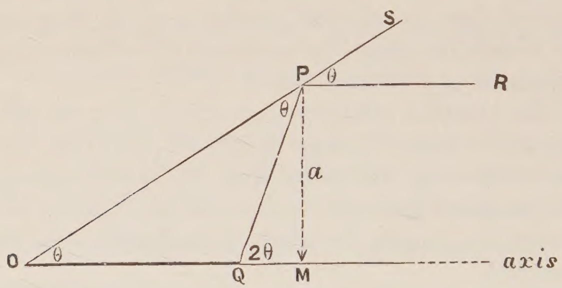
(6) THEOREM III. *A hollow cone along whose axis lies a cylindrical selenium-cell of given length will reflect onto that cylindrical surface the greatest number of rays* (that traverse space parallel to the axis) *if its angular semi-aperture be* 45°.

The calculation amounts to finding the angle that will, with

a given length of cell, give the greatest possible linear aperture.

In the figure 1, let POM represent the angle of half-aperture, which we will call θ. Let O Q ($=l$) be the length of cylinder, which may be supposed to be thin. Let the ray R P, P Q, be drawn making equal angles of incidence and reflexion with the surface of the cone. Then, since P R is parallel to the axis of the cone, the angle S P R$=$Q P O$=\theta$, and the triangle O Q P is isosceles. Hence the exterior angle P Q M$=2\theta$. If P M be drawn from P perpendicular to the axis, its length, which we may call a, will be that of the half-aperture.

Fig. 1.



Now $$\sin \mathrm{P\,Q\,M} = \sin 2\theta = \frac{a}{\mathrm{P\,Q}};$$

therefore $$a = \mathrm{P\,Q} \sin 2\theta,$$

$$a = l \sin 2\theta.$$

Hence if l be constant, a will be a maximum when $\frac{da}{d\theta} = 0$.

Now $\frac{da}{d\theta} = l \cos 2\theta$; and equating to zero we find

$$\cos 2\theta = 0,$$

$$2\theta = 90^\circ,$$

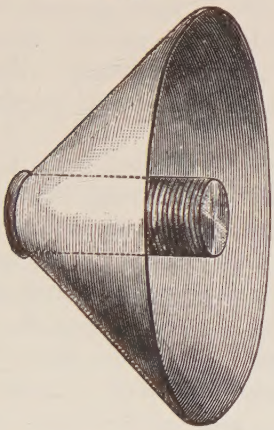
or $$\theta = 45^\circ.$$

In other words, the mirror cone must have an apparent vertical angle of 90°, and its development will be a sector of 254° 33′·6 $\left(=\frac{360^\circ}{\sqrt{2}}\right)$ cut from a circle whose radius is $\sqrt{2} \times$ O Q.

If the cylinder cell have itself a radius r, then the whole diameter of the linear aperture will be equal to $2\ (l+r)$; and the cone may be conveniently truncated at a distance along the axis from O equal to r, which would leave a circle of $2r$ diameter just fitting the posterior end of the cylinder.

It may be remarked that the anterior end of the cylinder will, when it is placed in position, be in the same plane as the circular mouth of the mirror cone, and the general appearance of the mirror and cylinder will be that presented in figure 2.

Fig. 2.



With an angular aperture less than 90°, the depth of the mirror from back to front must be greater than the length of the cylinder; and the mirror, however prolonged, could not bring more rays to the surface of the cylinder except they underwent more than one reflexion.

If the angular aperture should be greater than 90°, the diameter of the cone that will reflect the effective rays will be less than that of the 90° cone, and hence cannot gather as much light.

One advantage possessed by such a mirror cone of the form specified above over any other form, parabolic or otherwise, is that all the rays meet the sensitive surface of the cylinder at normal incidence, and the loss by reflexion will be therefore a minimum.

(7) In preparing to repeat the Photophone experiments, the author has constructed sundry cells in a manner somewhat differing from that adopted by Professor Bell.

Finding it laborious to cut and fix the alternate disks of mica and brass, he constructed a cell by winding brass wires spirally round a glass tube so that the successive convolutions did not quite touch. Selenium was afterwards applied in the interstices, and alternate convolutions were connected

metallically, the wires being cut and then soldered with alternate junctions. Afterwards two parallel wires, wound side by side as in a double-threaded screw, were employed. One of these cells was found by the author on Oct. 19th to have as small a resistance (in the dark) as 240 ohms. On account, however, of the wire not adhering firmly to the glass, and from other causes, the arrangement, though far more easily constructed than the built-up cell, was not satisfactory. Taking a hint from Mr. Shelford Bidwell, who has recently published a communication on the Photophone in 'Nature,' the author has constructed cylinders of slate grooved with a fine double-threaded screw, in which the parallel wires are laid. These cells prove much more satisfactory. Experiments are now proceeding with cells of this kind varying in length from two to eight inches.

(8) The theorems enunciated above concerning the advantages of enlarging the size of the selenium-cell can readily be put to experimental test with cells such as described. There is of course a practical limit beyond which further increase of size will be of no advantage—such a limit being determined in particular cases by the resistance of the telephones and of the telephone-circuit, and by the other conditions, electrical and optical, of the experiment.

University College, Bristol,
Nov. 24, 1880.

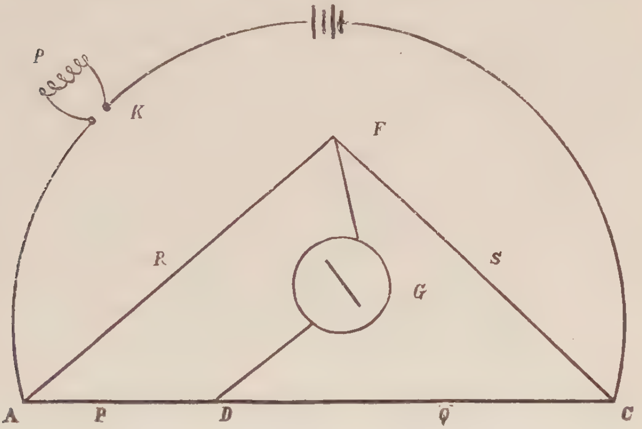
XIX. *On the Measurement of Small Resistances. By* R. T. Glazebrook, *M.A., Fellow of Trinity College, and Demonstrator of Experimental Physics at the Cavendish Laboratory, Cambridge**.

While measuring some small resistances with a Wheatstone's bridge at the Cavendish Laboratory, Mr. W. B. Allcock observed that the apparent measure of the resistance varied with the resistance in the battery circuit.

Let A D C be the bridge-wire, D being the point at which the sliding contact is to be made. Let P be the resistance of A D, Q that of D C (P and Q include the resistances added

* Read January 22, 1881.

to the ends of the bridge-wire), A F the resistance R to be measured, F C the standard S with which R is to be compared. At K in the battery circuit is a key.



In one position of the key the battery-current has to pass through the resistance ρ ; in the other the ends of the coil ρ are connected, and it is thrown out of the circuit.

The experiment is as follows:—Place the key so that the resistance ρ is shunted, and adjust the position of D until there is no current through the galvanometer. Raise the key, and so throw ρ into the battery circuit; then the galvanometer-needle is considerably deflected, if the other resistances be small compared with ρ.

The experiment may of course be conducted in the reverse way, the position of equilibrium being found when ρ is in circuit. On depressing the key and shunting ρ the needle is deflected.

The deflection in this case is much greater than in the former; but then the battery-current is increased many times by the operation, and the apparatus therefore is rendered more sensitive; so that part of the effect may be due to a slight error in the original position of D.

The effect was reversed by changing the direction of the battery-current. This last fact showed that it could not spring from any heating of the coils. Moreover any heating-effect would be due to the difference in the temperature-coefficients of

R and S or P and Q respectively; but R and S were throughout as nearly as possible equal and similar, as also were P and Q.

But it is well known that a thermoelectric effect is produced by the contact at D. This, as will shortly be shown, will explain the phenomenon; and the effect may be made a means of measuring, approximately at least, the electromotive force set up at the junction. For let E be the E.M.F. of the battery, e that at the junction; then we may show that the current through the galvanometer is

$$x=\frac{E(QR-SP)+e\{B(Q+R+S+P)+(P+Q)(S+R)\}}{D} \quad (1)$$

B being the resistance in the battery circuit, and D a function of the resistances with which we are not at present concerned.

The condition for no current, therefore, is

$$0=E(QR-SP)+e\{B(Q+R+S+P)+(P+Q)(S+R)\},$$

and in the case in which the resistances to be measured are very nearly equal we may put $P=Q$, $R=S$, in the coefficient of e, so that

$$\frac{P}{Q}=\frac{R}{S}+\frac{e}{E}\left\{2B\left(\frac{1}{R}+\frac{1}{P}\right)+4\right\}. \quad (2)$$

Thus if B is large compared with R and P, the term involving e is appreciable, and the usual condition $P:Q=R:S$ is not sufficiently nearly true to give correct results.

The equation shows that the sign of the correction changes with that of E.

Let P' and Q' be the values of P and Q when the battery resistance is large.

Then

$$\frac{P'}{Q'}=\frac{P}{Q}+\frac{e}{E}\left\{2B\left(\frac{1}{R}+\frac{1}{P}\right)+4\right\},$$

and

$$e=\frac{E\left(\frac{P'}{Q'}-\frac{P}{Q}\right)}{2B\left(\frac{1}{R}+\frac{1}{P}\right)+4}. \quad (3)$$

With the view of testing the truth of this explanation, I

made a series of experiments, using a wire bridge and various small resistances.

The resistance of the bridge-wire in the experiments was ·08 ohm ; the wire was a metre long, and graduated to millimetres.

Different observations for the value of P agree to a fraction of a millimetre of the bridge-wire, while the differences in the values of P′ never amounted to more than 4 millimetres, and were usually much less. The value of ρ was 150 ohms. In some of the experiments Leclanché cells were used, in others Daniells. The E.M.F. of the Leclanché cells was found by the potentiometer method to be about 1·25 Daniell.

In the results given in the table, the E.M.F. of a Daniell cell is taken as 1 volt. The battery resistance is neglected compared with the 150 ohms interposed.

Table giving Results of Experiments.

No. of cells.	Bridge-reading for P.	Bridge-reading for P′.	R in ohms.	Resistance added to arms of bridge.	e in volts.
	mm.	mm.			
3 Leclanchés ...	501·2	504·9	2·02	0	$75 \cdot 10^{-7}$
2 do., multiple arc	501·2	510·2	2·02	0	$60 \cdot 10^{-7}$
1 Leclanché......	496·1	472·9	·04	0	$75 \cdot 10^{-7}$
1 Leclanché......	544·6	526·3	2·02	2·2	$65 \cdot 10^{-7}$
1 Leclanché......	705·5	644·0	2·02	5·2	$63 \cdot 10^{-7}$
1 Daniell	500·8	467·5	·02	0	$60 \cdot 10^{-7}$
1 Daniell	507·5	489·0	·04	0	$63 \cdot 10^{-7}$

Thus the mean value of e as given by the experiments with the Leclanché cells is $77{\cdot}6 \times 10^{-7}$ volt, while that as given by the Daniells is $61{\cdot}5 \times 10^{-7}$ volt. The wire of the bridge and the contact piece are platinum-iridium, the other wires are copper.

The closeness in the values of e would appear to show that the assumption of some action at this junction will account for most of the phenomena ; while the constancy of the effect shows that, if due to a difference of temperature between the bridge-wire and the contact piece, that difference cannot vary much.

The hand of the experimenter was separated from the metal of the contact piece by a cap of wood. The effect of maintaining contact for some time after finding the equilibrium-position was tried without result. If the junctions A and C of the platinum-wire and the battery circuit be heated, the effect is decreased; if they be cooled, it is increased. Thus the effect, if thermoelectric, is due to heating at D.

In making measurements of resistance, this effect can of course be eliminated in various ways; it appears, however, to be worth while to call attention to its existence. One method frequently adopted to correct for this thermoelectric effect is given by Prof. Chrystal and Mr. Saunders (Report on the Standard Units of Resistance, B.A. Report 1876, p. 13). They made contact with the sliding piece before making the battery-contact, and waited until the thermoelectric current had attained a steady value and the galvanometer-needle come to rest in a position slightly disturbed from its original one. If on making the battery-contact no effect was produced, the resistance is given by the ordinary equation $P : Q = R : S$. This of course is only applicable when none of the coils used have any appreciable self-induction.

Another method is to reverse the battery-current.

Let $P_1\, Q_1$ be the values of $P\, Q$ when the current is in one direction, $P_2\, Q_2$ when it is in the other. Then the true value of $\frac{R}{S}$ is

$$\tfrac{1}{2}\left(\frac{P_1}{Q_1}+\frac{P_2}{Q_2}\right).$$

A third method, applicable to coils with no self-induction, is to interchange the battery and the galvanometer. The thermoelectromotive force is then superposed on that of the battery, and the condition $PS = QR$ holds. The most serious objection to this seems to be that the whole current must pass through the sliding junction, which may thus get considerably heated, and so damage the bridge-wire. Perhaps the simplest correction is to keep the battery resistance low compared with the others.

I tried the effect of using copper wire for the bridge in order to have only one metal throughout; but I found that the contact between my fingers of two pieces of copper wire

of somewhat different thickness produced a very appreciable current.

In the experiments given above the E.M.F. produced is about equal to that of a copper and platinum couple in which the difference of temperature is between 2° and 3°.

I was assisted in making the experiments by Mr. E. B. Sargant, Scholar of Trinity College.

XX. *Determination of the Density of Fluid Bismuth by means of the Oncosimeter. By* Professor W. CHANDLER ROBERTS, *F.R.S., and* THOMAS WRIGHTSON, *Memb. Inst. C.E.**

SOME time since, one of us described the results of experiments made to determine the density of metallic silver and of certain alloys of silver and copper when in a molten state†. The method adopted was that devised by Mr. R. Mallet‡; and the details were as follows:—A conical vessel of best thin Low-Moor plate (1 millim. thick), about 16 centims. in height, and having an internal volume of about 540 cubic centims., was weighed, first empty, and subsequently when filled with distilled water at a known temperature. The necessary data were thus afforded for accurately determining its capacity at the temperature of the air. Molten silver was then poured into it, the temperature at the time of pouring being ascertained by the calorimetric method. The precautions, as regards filling, pointed out by Mr. Mallet, were adopted; and as soon as the metal was quite cold, the cone with its contents was again weighed.

Experiments were also made on the density of fluid bismuth; and two distinct determinations gave the following results:—

$$\left.\begin{matrix}10\cdot005\\10\cdot072\end{matrix}\right\}\text{ Mean } 10\cdot039.$$

The invention of the oncosimeter§ appeared to afford an opportunity for resuming the investigation on a new basis, more especially as the delicacy of the instrument had already

* Read February 12, 1881.

† Roberts, Proc. Roy. Soc. vol. xxiii. p. 493.

‡ Proc. Roy. Soc. vol. xxii. p. 366, and vol. xxiii. p. 209.

§ Wrightson, Journ. Iron and Steel Inst. No. II. 1879, p. 418.

been proved by experiments on a considerable scale for determining the density of fluid cast iron. The following is the principle on which this instrument acts.

If a spherical ball of any metal be plunged below the surface of a molten bath of the same or another metal, the cold ball will displace its own volume of molten metal. If the densities of the cold and molten metal be the same, there will be equilibrium, and no floating or sinking effect will be exhibited. If the density of the cold be greater than of the molten metal, there will be a sinking effect, and if less a floating effect, when first immersed. As the temperature of the submerged ball rises, the volume of the displaced liquid will increase or decrease according as the ball expands or contracts. In order to register these changes the ball is hung on a spiral spring, and the slightest change in buoyancy causes an elongation or contraction of this spring which can be read off on a scale of ounces, and is recorded by a pencil on a revolving drum. A diagram is thus traced out the ordinates of which represent increments of volume, or, in other words, of weight of fluid displaced—the zero-line, or line corresponding to a ball in a liquid of equal density, being previously traced out by revolving the drum without attaching the ball of metal itself to the spring, but with all other auxiliary attachments. By a simple adjustment the ball is kept constantly depressed to the same extent below the surface of the liquid; and the ordinate of this pencil-line, measuring from the line of equilibrium, thus gives an exact measure of the floating or sinking effect at every stage of temperature, from the cold solid to the state when the ball begins to melt.

If the weight and specific gravity of the ball be taken when cold, we have, with the ordinate on the diagram at the moment of immersion, sufficient data for determining the density of the fluid metal; for

$$\frac{W}{W'}=\frac{D}{D'},$$

the volumes being equal. And, remembering that

$$W \text{ (weight of liquid)} = W' \text{ (weight of ball)} + x$$

(where x is always measured as a $+$ve or $-$ve floating effect), we have

$$D=\frac{D' \times (W'+x)}{W'}.$$

The following table shows the results of six experiments to determine the density of fluid bismuth made by the authors in the laboratory of the Royal Mint. The bismuth was kept just above its melting-point; and this was ensured by placing pieces of metal in the molten mass, which were observed just to melt.

No. of exp.	Diameter of ball, in inches.	Weight, in troy ounces, including the stem for attachment.	Specific gravity of cold ball, including this stem.	Floating effect on first immersion, in troy ounces.	Deduced specific gravity of fluid metal.	Remarks.
1.	2	23·33	9·72	1·0	10·13	Bismuth ball in fluid bismuth.
2.	2·25	33·46	9·755	1·3	10 11	do.
3.	do.	33·37	9·757	0·6	9·94	do.
4.	do.	33·53	9·774	0·7	9·98	do.
5.	do.	22·184	6·99 (iron)	9·3	9·92	Iron ball in fluid bismuth.
6.	do.	22·184	7·02 (do.)	10·2	10·25	do.
				Mean	10·055	
			Specific gravity of solid bismuth		9·82	

It will be seen that, considering the difficulties of manipulation, the results are remarkably concordant, and their mean agrees very closely with that obtained by Mallet's method. We venture to think, therefore, that the density of bismuth in the solid and the fluid state may now be considered to be definitely settled.

Fig. 1 is the oncosimeter diagram of experiment No. 2 (see table), with a calculation of the fluid specific gravity annexed. When first immersed, the floating effect is 1·3 troy ounce, which (with the weight and density of the ball known) is all we require to determine the fluid-density. Bismuth has a low heat-conducting power; and therefore the mass of the ball is reduced by surface-melting before much heat can penetrate to

Fig. 1.

Oncosimeter Diagram of Bismuth Ball, 2·25 inches diameter, immersed in Fluid Bismuth (see table, Exp. 2).

Weight of ball and immersed part of stalk ...... 33·46 oz.
Specific gravity of do. do. ...... 9·755
First floating effect .... 1·3 oz.

$$\text{Specific gravity of fluid} = \frac{34{\cdot}76 \times 9{\cdot}755}{33{\cdot}46} = 10{\cdot}11.$$

the centre. Hence the diagram does not accurately show, as in metals of high conducting-power, the change of volume, the effect being compounded with that produced by loss of mass.

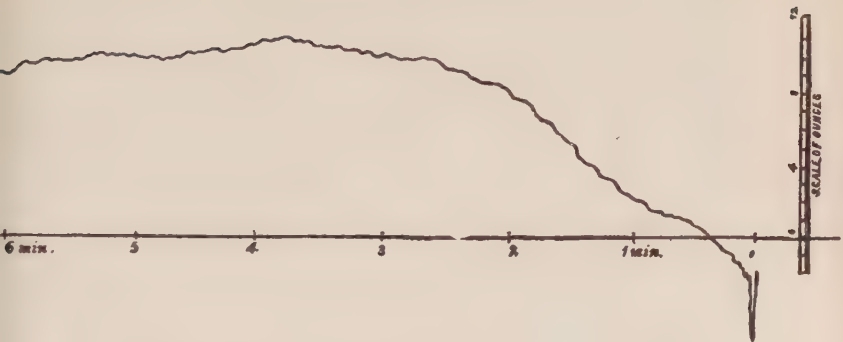
In the case of iron, the conducting-power of which is high, diagrams taken with the oncosimeter show correctly the expansion of the ball until it is in a uniformly plastic state. Fig. 2 is a diagram of a cast-iron ball, 4 inches diameter, immersed in fluid iron of the same quality (No. 4 Cleveland). In this the solid iron is shown to be of less volume than the liquid for about 25 seconds, then to rise gradually in volume until in 4 minutes it becomes plastic (this having been proved by taking balls out in this stage of temperature, when an iron pin could be run through and through the metal as though it were a piece of putty). The slight fall of the line for about 2 minutes towards the left of the diagram probably shows a slight loss of mass owing to surface-melting; and then the whole ball melts with great rapidity, and as it joins the liquid metal of the bath the line of volume shoots rapidly down to the equilibrium-line. This diagram shows, up to the plastic point (where loss of mass commences), the gradual change of volume at progressive intervals of time.

The diagram read from left to right should represent the change from liquid to solid; and this is quite in accordance with other observations on cast iron*.

* Journ. Iron and Steel Inst. No. II. 1879, and No. I. 1880.

Fig. 2.

Experiment No. 23, on Iron.



Weight of ball and immersed part of stalk............	132 oz.
Specific gravity of ball and immersed part of stalk	6·95
Maximum sinking effect...................................	2 oz.
Maximum floating effect	11 oz.

Specific gravity of fluid iron $= \dfrac{6{\cdot}95 \times 130{\cdot}0}{132} \ldots = 6{\cdot}84$

Specific gravity of plastic metal $= \dfrac{6{\cdot}95 \times 132}{145{\cdot}00} \ldots = 6{\cdot}33$

4-inch ball of No. 4 foundry iron (Cleveland). Run with very hot metal. Immersed in No. 4 foundry iron.

According to these experiments, iron expands rapidly (as much as 6 per cent.) in cooling from the liquid to the plastic state, and then contracts 7 per cent. to solidity; whereas bismuth appears to expand in cooling from the liquid to the solid state about 2·35 per cent.

XXI. *An Integrating-Machine.*

By C. V. Boys, *Assoc. Royal School of Mines.**

[Plate XI.]

All the integrating-machines hitherto made of which I can find any record may be classed under two heads:—one, of which Amsler's beautiful instrument is the sole representative, depending on the revolution of a disk which partly rolls and partly slides on the paper; the other, comprising

* Read February 26, 1881.

all the remaining machines, depending on the varying diameters of the parts of a rolling system. As this subject has been treated so recently by Mr. Merrifield in his "Report on the Present State of Knowledge of the Application of Quadratures and Interpolation to Actual Data," read at the meeting of the British Association at Swansea, 1880, in which he briefly describes previous machines and refers to the papers in which a full description may be found, I do not think it advisable to say more concerning them, except that none of them do their work by the method of the mathematician, but in their own way. The machine, however, which I have the honour of bringing before the notice of the Physical Society is an exact mechanical translation of the mathematical method of integrating $y\,dx$, and thus forms a third type of instrument.

The mathematical rule may be described in words as follows:—Required the area between a curve, the axis of x, and two ordinates. It is necessary to draw a new curve such that its steepness, as measured by the tangent of the inclination, for any value of x may be proportional to the ordinate of the given curve for the same value of x. The *ascent* then made by the new curve in passing from one ordinate to the other is a measure of the area required.

On Plate XI. is a plan and side elevation of a model of the instrument made merely to test the idea: the arrangement of the details is not altogether convenient. The framework is a kind of T-square carrying a fixed centre B, which moves along the axis of x of the given curve; a rod passing always through B carries a pointer A, which is constrained to move in the vertical line $e\,e$ of the T-square; A then can be made to follow any given curve. The distance of B from the edge $e\,e$ is constant; call it k: therefore the inclination of the rod AB is such that its tangent is equal to the ordinate of the given curve divided by k; that is, the tangent of the inclination is proportional to the ordinate; therefore, as the instrument is moved over the paper, AB has always the inclination of the required curve.

The part of the instrument that draws the curve is a three-wheeled cart of lead whose front wheel F is mounted, not as a castor, but like the steering-wheel of a bicycle. When such a cart is moved, the front wheel F can only move in the

direction of its own plane, whatever be the position of the cart; if, therefore, the cart is so moved that F is always in the line *e e* and at the same time has its plane parallel to the rod A B, then F must necessarily describe the required curve; and if it is made to pass over a sheet of black tracing-paper, the required curve will be drawn.

The upper end of the T-square is raised above the paper, and forms a bridge, under which the cart travels. There is a longitudinal slot in this bridge, in which lies a horizontal wheel, carried by that part of the cart corresponding to the head of a bicycle; by this means the horizontal movement communicated to the front wheel of the cart by the bridge is equal to that of the pointer A; at the same time the cart is free to move vertically. It only remains to describe the mechanism which causes the plane of the front wheel of the cart to be always parallel to A B. For this purpose I make use of the principle of the epicyclic train. If three equal wheels are mounted on an arm, with their centres in a straight line and their edges in contact, any motion may be given to the arm or to the first wheel, *a*, yet lines on the first and last wheels, *a* and *b*, if ever parallel, will always be so. Instead of the middle wheel an open band may connect the two outside wheels, with the same result. In the same way another arm may carry a third wheel, *c*, connected with *b* by an open band, as shown; then whatever motion is given to the wheel *a* or to the arms, lines on *a* and *c*, if ever parallel, will always be so. To apply this principle the wheel *a* is mounted on the fixed centre B, and its rotation is equal to that of the rod A B. A pair of arms hinged at H connect B with that part of the cart corresponding to the head of a bicycle, while

Fig. 1.

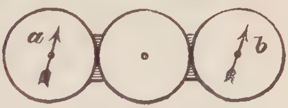
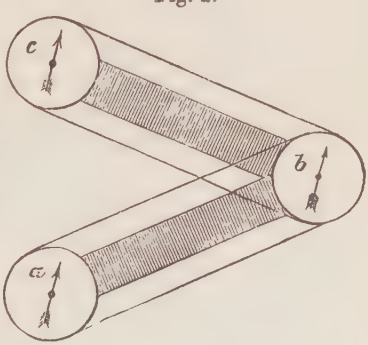


Fig. 2.



the handles are replaced by the wheel c; the wheel b is mounted on the hinge H, and a single band goes round all three wheels, as shown. The wheel b and the arms are balanced about B by the counterpoise W. Thus, as A traces out any given curve, the front wheel of the cart has its plane always parallel to A B; and as the connexion between the cart and B in no way interferes with its vertical motion, the front wheel must describe the required curve. As the ascent in this curve is equal to $\frac{1}{k}\int y\,dx$, all that has to be done is to measure the ascent, multiply by k, and the product is the area required. If A is taken round any closed curve, the ascent can be measured immediately by a rule and multiplied by k as before.

In the instrument shown, k can be made either one, two, three, π, or $\frac{\pi}{2}$ inches. If the one-inch constant is used, the inclination of the rod A B is with large ordinates so great and the motion of the cart so nearly at right angles to the direction in which it is pushed, that there is danger of its being upset. This difficulty is in great measure avoided by inclining the board in the direction $e\,e$; and then, as the inclination of A B becomes greater and the power of the instrument to drive the cart becomes less, the action of gravity on the cart increases, and it can be moved with equal ease in all directions.

As the model works exceedingly well, I have no doubt that a carefully-made machine would give results as accurate as any other planimeter. As an aid in teaching physics to pupils not familiar with the principles of the integral calculus, and in illustrating those principles themselves, I think it would be found of very great value. To justify myself for bringing a subject purely mathematical before this Society, I will briefly give a few examples of its use. For simplicity's sake let $k=1$.

If A is moved along the axis of x (that is, $y=0$), the cart draws a horizontal line, the ascent is nothing, and the area is nothing. By this means any want of parallelism between the front wheel F and AB can be detected and set right (see Plate XI. fig. 1).

If A is moved along a line parallel to the axis of x (that is, $y=c$), the cart draws the straight line $y=cx$; that is, the

inclination is constant, showing that area is passed over uniformly (see Plate XI. fig. 1).

If A is moved along an inclined straight line $y=cx$, the cart draws the parabola $y=\frac{cx}{2}$ (see Plate XI. fig. 2). This is the path of a projectile: and the machine proves that it must be so; for taking abscissæ as time, the curve representing the velocity of falling is an inclined straight line, while the space fallen in any time, being measured by the area between the inclined line and the axis of x up to that point, is found by the cart; and as the horizontal movement is proportional to the time, the curve drawn by the cart is the path of a projectile.

If A is moved along the curve $y=\frac{1}{x^2}$, a curve representing attraction, the cart draws a rectangular hyperbola, showing that potential varies inversely as distance. As abscissæ are distances and ordinates forces, it is plain that the work done by an attracting body in bringing a unit from an infinite distance up to a point (that is, the potential at that point) is measured by the area between the curve, the axis of x, and the ordinate at that point; and as in finding this area the machine draws a rectangular hyperbola in which, of course, y varies inversely as x, it proves that potential varies inversely as distance (see Plate XI. fig. 3).

If A is moved along the logarithmic curve $y=e^x$, the cart draws an identical curve; and this it should do, since $\frac{de}{dx}=e^x$ (see Plate XI. fig. 4). Since the pointer A and the cart describe identical curves, it is plain that their distance asunder is constant; if, therefore, these two are connected by a link, and then the machine is started on the axis of x, they will each describe a horizontal line. But this will be an unstable motion; for if they depart ever so little from horizontal motion, they will turn aside faster and faster, the cart pulling the pointer and the pointer directing the cart, and thus originate the logarithmic curve.

If A is moved along a wave-line symmetrically placed with respect to the axis of x, the cart draws another wave-line a quarter of a wave-length behind the first in point of time. If

the first line represents the varying strength of an induced electrical current, the second shows the nature of the primary that would give rise to such a current (see Plate XI. fig. 5).

Fig. 6 shows the application of the machine to the determination of the area of a closed curve.

The rules for finding maxima and minima and points of inflexion are rendered obvious by manipulating the machine. By no means can the cart be made to trace a maximum or a minimum unless the pointer A *cross* the axis of x; nor can it pass a point of inflexion unless A pass a maximum or a minimum.

An indefinite integral requires the addition of a constant; but on integrating between limits this constant goes out. This is illustrated by the fact that the cart may be started on any level on the board, but the ascent made is the same.

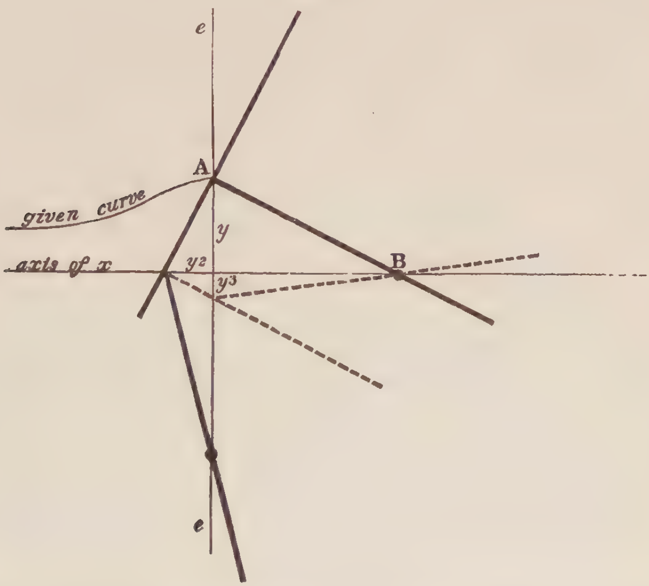
Not only does the machine integrate $y\,dx$, but if the plane of the front wheel of the cart is set at right angles instead of parallel to AB, then the cart finds the integral of $\frac{dx}{y}$, and thus solves problems such, for instance, as the time occupied by a body in moving along a path when the law of the velocity at different parts is known. This is evidently true; for if a line be drawn perpendicularly to AB through B, it will cut $e\,e$ at a point distant from the axis of x by an amount equal to $\frac{1}{y}$, and therefore its inclination is such that its tangent is equal to $\frac{1}{y}$. As the cart travels down instead of up for positive values of y, its descent instead of its ascent must be taken.

Some modifications in the instrument would enable it to integrate y^2dx or y^3dx; it could also be made to integrate the product of two or more functions. I do not intend to go into details with regard to these extensions of the machine, but merely to explain the principle that would be employed. As before, let $k=1$. To integrate y^2dx the rod AB would be replaced by a T, as shown in fig. 3. The head of this would obviously cut the axis of x in advance of the edge $e\,e$ by an amount equal to y^2. Let a rod pass through this point of intersection and through a point on $e\,e$ distant from the

axis of x by an amount equal to k or 1, then the angle between this rod and $e\,e$ is such that its tangent is equal to y^2; and if the plane of the front wheel of the cart be kept at right angles to this rod, the cart will integrate y^2dx.

To integrate y^3dx the second rod would be kept parallel to A B, and the point where it cut $e\,e$ would be distant from the axis of x by an amount equal to y^3. If then the plane of the front wheel of the cart were kept parallel to the line connecting this point with B, as shown by the dotted

Fig. 3.



lines in fig. 3, the machine would integrate y^3dx. In these cases, as in the integration of products, it would be well to make k so large that the ordinates should never much exceed it.

To integrate the product of two functions—that is, to find $\int\phi x\,\psi x\,dx$, the two curves $y=\phi x$ and $y=\psi x$ would have to be drawn about two axes of x, one above the other, and two tracing-points, each on the line $e\,e$, would follow the curves. The fixed centre B would pass over the lower axis of x; but the epicyclic connexion, instead of joining B with the cart,

would connect B with the upper tracing-point, and cause a rod passing through this point to be always at right angles to A B. This upper rod would cut the upper axis of x at a point distant from $e\,e$ by an amount equal to $\phi x\,\psi x$; if, then, the front wheel of the cart is kept at right angles to a line joining this point of intersection with a point on $e\,e$ distant from the upper axis of x by an amount equal to k or 1, then the cart will draw the line $y=\int_0^x \phi x\,\psi x\,dx$. The same principle might be applied to integrate the product of more than two functions.

As in the case of simple functions, so with squares, cubes, and products, the reciprocal could be integrated by twisting the plane of the front wheel of the cart through a right angle. Also the integral of $\frac{\phi x}{\psi x}$ could be found by making the lower curve $y=\psi x$, the upper curve $y=\phi x$, and by keeping the rod which passes through the upper tracing-point parallel instead of at right angles to A B.

The axis of x is drawn by using a little T square of such a length that its end is the same distance from the edge of the board as the fixed centre B; then a pencil held at the end while the square is moved across the board will at once give the axis of x.

I do not consider the modifications of the machine to be of much importance; but the simple machine as described in the first part of this paper, is, I think, interesting as being the only one that really illustrates the mathematical formula for the integration of $y\,dx$.

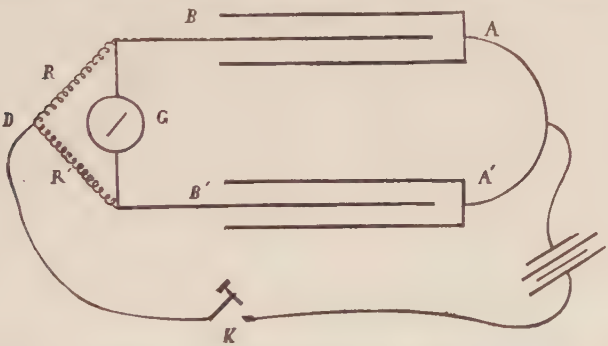
My thanks are due to Mr. Hilger, of Tottenham Court Road, for the great care and skill which he has bestowed upon the construction of the instrument. Certain modifications of detail, however, have been introduced which make it more easy of manipulation without sacrifice of simplicity.

Physical Laboratory,
South Kensington.

XXII. *On a Method of Comparing the Electrical Capacities of two Condensers.* By R. T. Glazebrook, *M.A., Fellow of Trinity College, and Demonstrator of Experimental Physics at the Cavendish Laboratory, Cambridge* *.

The following is a well-known method of comparing the capacities of two condensers:—

Let A, A′ be the outer coatings, B, B′ the inner of two condensers. Connect A A′ together, and to one pole of a battery. Connect B to a resistance R and to one pole of a



galvanometer, B′ to another resistance R′ and to the other pole of the galvanometer. Connect the other ends of R R′ together and to a key K, and let the second screw of the key be in connexion with the other pole of the battery. Let C C′ be the capacities of the condensers. On depressing the key the condensers are charged; and it is easy to show that, if $CR = C'R'$, no current passes through the galvanometer.

If, then, we adjust R until no current is observed on making contact, R′ remaining unaltered, we can find the ratio of C to C′.

I propose to discuss the more general problem of finding the current through the galvanometer when the equation $CR = C'R'$ is not fulfilled, and hence to obtain the conditions of sensibility.

Let V_1 be the potential of A, V_2 of the other pole of the battery, V of B, V′ of B′, at time t. Let G be the galvanometer resistance.

* Read January 22, 1881.

Let ι = current in R;

ι' = „ R′;

ι_1 = current into condenser A;

ι_1' = „ „ B;

x = current through galvanometer;

Q, Q′ = the quantities in the condensers.

Let us further suppose that there is a small leakage through the condensers, $\rho\,\rho'$ being their resistances.

Then we have

$$\left.\begin{aligned}\iota &= \frac{V_2-V}{R}, \quad \iota'=\frac{V_2-V'}{R'}, \quad x=\frac{V-V'}{G},\\ \iota_1 &= \iota-x, \quad \iota_1'=\iota'+x,\\ Q &= C(V-V_1), \quad Q'=C'(V'-V_1),\\ \iota_1 &= \frac{dQ}{dt}+\frac{V-V_1}{\rho},\\ \iota_1' &= \frac{dQ'}{dt}+\frac{V'-V_1}{\rho'},\end{aligned}\right\} \quad . \quad . \quad (1)$$

From these we obtain:—

$$Gx+R\iota-R'\iota'=0; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$CR\frac{d\iota}{dt}+\frac{R+\rho}{\rho}\left\{\iota-\frac{V_2-V_1}{R+\rho}\right\}-x=0; \quad . \quad . \quad . \quad (3)$$

$$C'R'\frac{d\iota'}{dt}+\frac{R'+\rho'}{\rho'}\left(\iota'-\frac{V_2-V_1}{R'+\rho'}\right)+x=0. \quad . \quad . \quad . \quad (4)$$

Assume

$$\begin{aligned}\iota-\frac{V_2-V_1}{R+\rho} &= Ae^{-nt},\\ \iota'-\frac{V_2-V_1}{R'+\rho'} &= A'e^{-nt},\\ x &= Be^{-nt}.\end{aligned}$$

On substituting we have

$$A\left\{\frac{R+\rho}{\rho}-CRn\right\}-B=0, \quad . \quad . \quad . \quad . \quad (5)$$

$$A'\left\{\frac{R'+\rho'}{\rho'}-C'R'n\right\}+B=0, \quad . \quad . \quad . \quad (6)$$

$$GB+RA-R'A'=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

whence

$$\left.\begin{aligned} &A\left\{G\left(\frac{R+\rho}{\rho}-RCn\right)+R\right\}-R'A'=0,\\ &A'\left\{G\left(\frac{R'+\rho'}{\rho'}-R'C'n\right)+R'\right\}-RA=0.\end{aligned}\right\} \quad . \quad (8)$$

Eliminating $A\,A'$ we arrive at the quadratic,

$$\begin{aligned} n^2-n&\left\{\frac{1}{G}\left(\frac{1}{C}+\frac{1}{C'}\right)+\frac{1}{R'C'}\left(\frac{R+\rho}{\rho}\right)+\frac{1}{RC}\frac{R'+\rho'}{\rho'}\right)\right\}\\ &+\frac{1}{GRR'CC'}\left\{\frac{G(R+\rho)(R'+\rho')}{\rho\rho'}+\frac{R(R'+\rho')}{\rho'}\right.\\ &\left.+\frac{R'(R+\rho)}{\rho}\right\}=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)\end{aligned}$$

Let $n_1\ n_2$ be the roots, and let

$$\left.\begin{aligned} &G\left(\frac{R+\rho}{\rho}-RCn\right)+R=\lambda,\\ &G\left(\frac{R'+\rho'}{\rho'}-R'Cn\right)+R'=\lambda'.\end{aligned}\right\} . \qquad (10)$$

Then from (8) we have

$$\left.\begin{aligned} \lambda_1A_1+\lambda_2A_2&=R'(A_1'+A_2'),\\ \lambda_1'A_1'+\lambda_2'A_2'&=R(A_1+A_2),\end{aligned}\right\} \quad . \quad . \quad (11)$$

where A_1, λ_1 &c. denote the values of A, λ corresponding to n_1, n_2.

Also initially

$$\iota=\frac{V_2-V_1}{R}, \qquad \iota'=\frac{V_2-V_1}{R'}; \quad . \quad . \quad . \quad . \quad (12)$$

therefore, putting $t=0$ and $V_2-V_1=E$ in the equations

$$\iota=\frac{V_2-V_1}{R+\rho}+A_1e^{-n_1t}+A_2e^{-n_2t}$$

$$\iota'=\frac{V_2-V_1}{R'+\rho'}+A_1'e^{-n_1t}+A_2'e^{-n_2t},$$

we find

$$\left.\begin{aligned} A_1+A_2&=\frac{E\rho}{R(R+\rho)},\\ A_1'+A_2'&=\frac{E\rho'}{R'(R'+\rho')}.\end{aligned}\right\} \quad . \quad . \quad . \quad (13)$$

Solving for A_1 A_2, we get

$$A_1=\frac{E}{R(n_1-n_2)}\left\{\frac{1}{GC}\left(\frac{\rho}{R+\rho}+\frac{\rho'}{R'+\rho'}\right)\right.$$

$$\left.+\frac{1}{R}\left(\frac{1}{RC}-\frac{n_2\rho}{R+\rho}\right)\right\}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

$$A_2=-\frac{E}{R(n_1-n_2)}\left\{\frac{1}{GC}\left(\frac{\rho}{R+\rho}-\frac{\rho'}{R'+\rho'}\right)\right.$$

$$\left.+\frac{1}{R}\left(\frac{1}{RC}-\frac{n_1\rho}{R+\rho}\right)\right\}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (15)$$

and similar values for A_1' A_2'.

Hence we find

$$Gx=E\left\{\frac{R'}{R'+\rho'}-\frac{R}{R+\rho}\right\}$$

$$+\frac{E}{n_1-n_2}\left[\left(\frac{1}{R'C'}-\frac{1}{RC}\right)(e^{-n_1t}-e^{-n_2t})\right.$$

$$+\left\{\left(n_2-\frac{1}{GC}-\frac{1}{GC'}\right)e^{-n_1t}-\left(n_1-\frac{1}{GC}-\frac{1}{GC'}\right)e^{-n_2t}\right\}$$

$$\left.\times\left\{\frac{\rho}{R+\rho}-\frac{\rho'}{R'+\rho'}\right\}\right]. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (16)$$

Let us take the case in which there is no leakage first, $\rho=\rho'=\infty$, and

$$x=\frac{E}{G(n_1-n_2)}\left(\frac{1}{R'C'}-\frac{1}{RC}\right)(e^{-n_1t}-e^{-n_2t}). \quad . \quad . \quad (17)$$

Thus, if $RC=R'C'$, we see that x is zero for all values of t.

The total effect on the galvanometer, since the time of charging is short, is proportional to the quantity of electricity which passes. To find this we integrate the value of x with respect to t from 0 to τ, and suppose τ so large that

$$e^{-n_1\tau}=e^{-n_2}=0.$$

Then, if P be the quantity,

$$P=\frac{E(R'C'-RC)}{GRR'CC'n_1n_2};$$

also

$$n_1n_2=\frac{G+R+R'}{GRR'CC'};$$

$$\therefore \quad P=\frac{E(R'C'-RC)}{G+R+R'}; \quad . \quad . \quad . \quad . \quad . \quad (18)$$

and if H be the strength of the field in which the needle hangs, T the time of a complete oscillation, k the galvanometer-constant, and α the angle through which the needle is turned,

$$P = \frac{HT}{\pi k} \sin\frac{\alpha}{2};$$

$$\therefore \quad \sin\frac{\alpha}{2} = \frac{\pi k}{HT} \cdot \frac{E(R'C'-RC)}{(G+R+R')}, \quad . \quad . \quad . \quad (19)$$

which leads to the condition that, when there is no throw of the galvanometer, $R'C'=RC$.

We proceed to inquire what resistances will give the most accurate value for the capacity C in terms of C′, the known capacity of a standard condenser when using a given galvanometer. Let us suppose the adjustment made by varying R, and determine the error δa produced in α by an error δR in R. Then, remembering that, when the adjustment is perfect, $RC=R'C'$ and $a=0$, if δR is the error from perfect adjustment, we have

$$\delta a = -\frac{2}{HT}\frac{E\pi k C\delta R}{(G+R+R')}; \quad . \quad . \quad . \quad . \quad (20)$$

and if δC is the error in the capacity, since $C=\dfrac{R'C'}{R}$,

$$\delta C = -\frac{R'C'\delta R}{R^2} = \frac{HT(G+R+R')R'C'}{2E\pi k CR^2}\delta a;$$

or, since $CR=C'R'$,

$$\delta C = \frac{HT(G+R+R')}{2E\pi k R}\,\delta a. \quad . \quad . \quad . \quad . \quad (21)$$

Now k varies as the number of turns in the galvanometer, and so also does G;

$$\therefore \quad K=\mu G,$$

$$\therefore \quad \delta C = \frac{HT\delta a}{2E\pi\mu}\left\{\frac{1}{G}+\frac{1}{R}+\frac{R'}{GR}\right\}; \quad . \quad . \quad (22)$$

and if we suppose that we are liable to an error δa in a, the error in C is least when the resistances R and R′ are both high.

Thus it is best to use, with a given galvanometer, high resistances R and R′.

We arrive at the same result if we make the adjustments by varying R′ instead of R.

Again, let us suppose that we have a galvanometer with a given channel, and we wish to fill it with wire so as to be most sensitive. Let V be the volume of the channel, y the radius of the wire, l its length, ρ its specific resistance, and suppose we neglect the thickness of the covering; then

$$4\rho y^2 = \mathrm{V},$$

$$\mathrm{G} = \frac{\rho l}{\pi y^2} = \frac{4\rho l^2}{\mathrm{V}},$$

$$k = g\rho,$$

where g depends only on the form and dimensions of the channel. We must therefore make

$$\frac{\frac{4\rho l^2}{\mathrm{V}} + \mathrm{R} + \mathrm{R}'}{l}$$

a minimum. We find

$$\frac{4\rho}{\mathrm{V}} - \frac{\mathrm{R}+\mathrm{R}'}{l^2} = 0; \quad . \quad . \quad . \quad . \quad . \quad (23)$$

and we get finally

$$\mathrm{G} = \mathrm{R} + \mathrm{R}'.$$

Now for a given value of G, R and R′ must be as high as possible; therefore we must make the resistance of our galvanometer as high as possible.

Returning to the general case in which there is a leakage in the condensers, and putting $t=\tau$, τ being so large that the terms involving $e^{-n\tau}$ may be neglected, we get

$$x = \frac{\mathrm{E}}{\mathrm{G}} \left\{ \frac{\mathrm{R}'}{\mathrm{R}'+\rho'} - \frac{\mathrm{R}}{\mathrm{R}+\rho} \right\}. \quad . \quad . \quad . \quad . \quad (24)$$

Thus there is a steady current through the galvanometer, and the needle is permanently deflected.

Again, if $t=0$, $x=0$. But let us suppose t very small, so that, on expanding e^{-nt}, powers higher than the first may be neglected, and find the initial current. We find

$$x = \frac{\mathrm{E}}{\mathrm{G}} \left\{ \frac{\mathrm{R}'\mathrm{C}' - \mathrm{RC}}{\mathrm{RC}\,\mathrm{R}'\mathrm{C}'} + \frac{1}{\mathrm{G}} \left(\frac{1}{\mathrm{C}} + \frac{1}{\mathrm{C}'} \right) \left(\frac{\rho}{\mathrm{R}+\rho} - \frac{\rho'}{\mathrm{R}'+\rho'} \right) \right\} t, \quad (25)$$

which reduces, if we neglect powers of $\frac{1}{\rho}$ above the first, to

$$x = \frac{\mathrm{E}}{\mathrm{G}} \left\{ \frac{\mathrm{R}'\mathrm{C}' - \mathrm{RC}}{\mathrm{RC}\,\mathrm{R}'\mathrm{C}'} + \frac{1}{\mathrm{G}} \left(\frac{1}{\mathrm{C}} + \frac{1}{\mathrm{C}'} \right) \left(\frac{\mathrm{R}'}{\rho'} - \frac{\mathrm{R}}{\rho} \right) \right\} t. \quad . \quad (26)$$

By altering R the sign of this may be made to change, and thus the initial current may be in the same or in the opposite direction to the final. In practice this is indicated by a short kick of the needle in one direction, followed by a deflection in the other.

On the same assumptions as to ρ, ρ', let us find the quantity of electricity which passes through the galvanometer in time τ, τ being very short, but yet so long that $e^{-n\tau}$ may be neglected. Integrating (16) and calling the quantity P, we have

$$P=\frac{E}{G+R+R'}\left[\{R'C'-RC\}\left\{1-\frac{G\left(\frac{R}{\rho}+\frac{R'}{\rho'}\right)+RR'\left(\frac{1}{\rho}+\frac{1}{\rho'}\right)}{G+R+R'}\right\}\right.$$
$$\left.-\left\{\frac{R'}{\rho'}-\frac{R}{\rho}\right\}\{RC+R'C'\}\right]+\frac{E}{G}\left(\frac{R'}{\rho'}-\frac{R}{\rho}\right)\tau. \quad . \quad (27)$$

If τ be very short, we may neglect the last term compared with the others, and, to the same degree of approximation with respect to ρ ρ, we get as the condition of no kick,

$$R'C'-RC=\left(\frac{R'}{\rho'}-\frac{R}{\rho}\right)(R'C'+RC),$$

or

$$\frac{RC}{R'C'}=1-2\left(\frac{R'}{\rho'}-\frac{R}{\rho}\right). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (28)$$

Again, let k, as before, be the galvanometer-constant, and δ the permanent deflexion. Then, from (25),

$$\frac{E}{G}\left\{\frac{R'}{\rho'}-\frac{R}{\rho}\right\}=k\tan\delta, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (29)$$

$$\frac{RC}{R'C'}=1-\frac{2Gk}{E}\tan\delta; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (30)$$

and this equation enables us to determine the capacity.

Let us suppose the adjustment made by varying R. Then, starting from a position in which the first kick is in an opposite direction to the final deflexion, adjust R until that kick is just reduced to zero, and the spot of light moves off gradually in the one direction, and after some oscillations comes to rest. Then, if δ is the deflexion of the galvanometer-needle the capacity is

$$C=\frac{C'R'}{R}\left\{1-\frac{2Gk}{E}\tan\delta\right\}.$$

Unless the leakage is considerable, the correction will be very small.

In measuring the capacity of many condensers, the difficulty is increased by the phenomenon of electric absorption. In fact the condenser has no true capacity; for the charge produced by a given electromotive force depends on the time during which that force has acted. We may, however, take the capacity as the ratio of the instantaneous charge to the electromotive force producing it; and in this case (contact with the battery being maintained only for a very short time) we may perhaps look on electric absorption as a kind of conduction through the substance of the condenser. We must suppose that the resistance to the conduction is a function of the time, which becomes indefinitely great after a not very long interval, but which we may perhaps treat as sensibly constant during the time for which contact is maintained; and if ρ_0, ρ'_0 be the values of this resistance during that interval, and, as before, we may neglect $\frac{1}{\rho_0^2}$ &c. and higher powers, we have, as the value of the capacity,

$$C=\frac{R'C'}{R}\left\{1-2\left(\frac{R'}{\rho'_0}-\frac{R}{\rho_0}\right)\right\}.$$

Thus a small correction should be applied to the value $\frac{R'C'}{R}$, depending on the rate of absorption during the interval for which contact is maintained with the battery. An approximation to this quantity may be obtained by charging the condenser for some time with a battery of known electromotive force, and then allowing it to discharge itself at small intervals of time through the galvanometer. On the whole, however, the results of measurements made, neglecting this correction, are fairly satisfactory.

The capacity of a paraffin condenser was determined by several observers during the past term at the Cavendish Laboratory. Their results differed by from $\frac{2}{3}$ to 1 per cent. The standard used was not in all cases the same; and the measures obtained by one observer, comparing this same condenser with two different standards, differed by about $\frac{1}{3}$ per cent. It seemed possible to determine within 10 ohms, when each of the resistances R, R′ was about 5000 ohms, the value of R for which the initial kick was zero.

XXIII. *Electric Absorption of Crystals.* *By* H. A. Rowland *and* E. H. Nichols, *of the Johns Hopkins University, Baltimore* *.

[Plate XII.]

I.

The theory of electric absorption does not seem to have as yet attracted the general attention which its importance demands; and from the writings of many physicists we should gather the impression that the subject is not thoroughly understood. Nevertheless the subject has been reduced to mathematics; and a more or less complete theory of it has been in existence for many years. Clausius seems to have been the first to give what is now considered the best theory. His memoir, "On the Mechanical Equivalent of an Electric Discharge," &c., was read at the Berlin Academy in 1852 †. In an addition to this memoir in 1866 he shows that a dielectric medium having in its mass particles imperfectly conducting would have the property of electric absorption. Maxwell, in his 'Electricity,' art. 325, gives this theory in a somewhat different form, and shows that a body composed of layers of different substances would possess the property in question. One of us, in a note in the 'American Journal of Mathematics,' No. 1, 1878, put the matter in a somewhat different form, and investigated the conditions for there being no electric absorption.

All these theories agree in showing that there should be no electric absorption in a perfectly homogeneous medium. A mass of glass can hardly be regarded as homogeneous, seeing that when we keep it melted for a long time a portion separates out in crystals. Glass can thus be roughly regarded as a mass of crystals with their axes in different directions in a medium of a different nature. It should thus have electric absorption. Among all solid bodies, we can select

* Read May 14th, 1881.

† I have obtained my knowledge of this memoir from the French translation, entitled *Théorie Mécanique de la Chaleur*, par R. Clausius, translated into French by F. Folie: Paris, 1869. The "Addition" does not appear in the memoir published in Pogg. *Ann.* vol. lxxxvi. p. 337, but was added in 1866 to the collection of memoirs.

none which we can regard as perfectly homogeneous along any given line through them, except crystals. The theory would then indicate that crystals should have no electric absorption; and it is the object of this paper to test this point. The theory of both Clausius and Maxwell refers only to the case of a condenser made of two parallel planes. In the "Note" referred to, one of us has shown that in other forms of condenser there can be electric absorption even in the case of homogeneous bodies. Hence the problem was to test the electric absorption of a crystal, in the case of an infinite plate of crystal with parallel sides. The considerations with regard to the infinite plate were avoided by using the guard-ring principle of Thomson.

The crystals which could be obtained in large and perfect plates were quartz and calcite. These were of a rather irregular form, about 35 millim. across and 1½ millim. thick, and perfectly ground to plane parallel faces. There were two quartz plates cut from the same crystal perpendicular to the axis, and two cleavage-plates of Iceland spar. There were also several specimens of glass ground to the same thickness; the plates were all perfectly transparent, with polished faces. Examined by polarized light, the quartz plates seemed perfectly homogeneous at all points except near the edge of one of them. This one showed traces of amethystine structure at that point; and a portion of one edge had a piece of quartz of opposite rotation set in; but the portion which was used in the experiment was apparently perfectly regular in structure. The fact that there are two species of quartz, right- and left-handed, with only a slight change in their crystalline structure, and that, as in amethyst, they often occur together, makes it not improbable that most pieces of right-handed quartz contain some molecules of left-handed quartz, and *vice versâ*. In this case quartz might possess the property of electric absorption to some degree. But Iceland spar should evidently more nearly satisfy the conditions. It is unfortunate that the two pieces of quartz were not cut from different crystals.

This reasoning was confirmed by the experiments, which showed that the quartz had about one ninth the absorption of glass; but that the Iceland spar had *none* whatever, and is

thus the first solid so far found having no electric absorption. Some crystals of mica &c. were tried; but calc spar is the only one which we can say, *à priori*, is perfectly homogeneous. Thus mica and selenite are so very lamellar in their character, that few specimens ever appear in which the laminæ are not more or less separated from one another; and thus they should have electric absorption.

II.

In the ordinary method of experimenting with the various forms of Leyden jar, there are, besides the residual discharge due to electric absorption in the substance of the insulator, two other sources of a return charge. The surface of the glass being more or less conducting, an electric charge creeps over the surface from the edges of the tinfoil. In discharging the jar in the usual way by a connecting wire, this surface remains charged, and the electricity is gradually conducted back to the coatings, and thus recharges them. If, furthermore, the coatings be fastened to the glass with shellac or other cement, the return charge may be partly due to it; for we have between the coatings not merely glass, but layers of glass, cement, &c., which the theory shows to give a residual discharge. Besides the coatings are not planes; and hence, as one of us has shown, there may be a return charge, even if the glass gave none between infinite planes. If the plates were merely laid on the glass without cementing, the same result would follow, since the insulator would then consist of air and glass in layers.

In the present research these were sources of error to be avoided, since the residual discharge due to the insulating plates themselves were to be compared. The condenser-plates were copper disks. These were amalgamated, so that there was a layer of mercury between them and the dielectric, which excluded the air and conducted the electricity directly to the surface of the dielectric: thus the condition of a single substance between the plates was fulfilled. The errors due to the creeping of the charge over the surface of the dielectric and that due to the plates not being infinite were avoided, the first entirely and the second partially, by the use of the guard-ring principle of Sir Wm. Thomson.

Fig. 1 (Pl. XII.) represents this apparatus. The plate of crystal, *c*, was placed between two amalgamated plates of copper, *a* and *b*, over the upper one of which the guard-ring, *d*, was carefully fitted; this ring, when down, served to charge and discharge the surface around the plate, *a*; and so the errors above referred to from the creeping of the charge along the plate, and from the plate not being infinite, were avoided.

The charging battery consisted of six large Leyden jars of nearly a square foot of coated surface each, charged to a small potential. Although accurate instruments were at hand for measuring the potential in absolute measure, it was considered sufficient to use a Harris unit-jar for giving a definite charge; for very accurate measurements were not desired, and the Harris unit-jar was entirely sufficient for the purpose. The return charge was measured by a Thomson quadrant-electrometer of the original well-known form.

The apparatus shown in fig. 1 performs all the necessary operations by a half turn of the handle *e*. By two half turns of the handle, one forward and the other back, the crystal condenser could be successively charged from the Leyden battery, discharged, the guard-ring raised, the upper plate, *a*, again insulated, and the connexion made with the quadrant-electrometer.

The copper ring, *d*, was suspended by three silk threads from the brass disk, *f*, which in turn could be raised and lowered by the crank, *g*. A small wire connected the ring with the rod on which was the ball *h*. This rod was insulated by the glass tube *i*, and could revolve about an axis at *k*. By the up-and-down motion of the rod the ball came into contact with the ball (*l*) connected with the earth, or the ball (*m*) connected with the battery. When the cranks were in the position shown in the figure, the heavy ball *n* caused the ball *h* to rise and press against *l*; but when *f* descended, the piece *o* pressed on the rod and caused *h* to fall on *m*.

Another rod, *q*, also more than balanced by a ball, *r*, was insulated by a glass tube, *s*, and connected with the quadrant-electrometer by a very fine wire. It could also turn around a pivot at *t*; so that when the ring *u* rested upon it, it fell on the upper condenser-plate *a*, and connected it with the electrometer: when the weight *u* was raised by the crank *v*,

the rod rested against *f*, and so connected the electrometer to the earth, to which the other quadrants were already connected.

At the beginning of an experiment, the insulating plate to be tested having been placed between the condenser-plates *a* and *b*, the handle was brought into such a position that the ring, *d*, rested on the plate around *a*. The lengths of the threads between *d* and *f* were such that *o* for this position of the handle did not touch *w*, and so *h* remained in connexion with the earth; and so *d* was also connected with the earth, and thus also with *b*. On now turning the handle further, the ball *h* descended to the ball *m*, and thus charged the condenser for any time desired. On now reversing the motion, the following operations took place:—

First, the ball *h* rose and discharged the condenser.

Second, the guard-ring *d* ascended.

Third, the rod *q*, which had been previously in contact with *p*, thus bringing the quadrant-electrometer to zero, now moved down and rested on the upper condenser-plate *a*. Thus any return charge quickly showed itself on the electrometer. The amount of deflection of the instrument depends upon the character of the dielectric, its thickness, the charge of the battery, the time of contact with the battery, and upon the length of time of discharging.

III.

In comparing the glass with the crystal plates, the electrometer was rendered as little sensitive as the ordinary arrangement of the instrument without the inductor-plate would allow. The electric absorption of the glass plates for a charge in the battery of two or three sparks from the Harris unit-jar then sufficed, after 20 or 30 seconds contact with the battery and 5 seconds discharging time, to give a deflection of about 200 scale-divisions, which were millimetres. The quartz and calcite plates were then alternately substituted for the glass, the same charge and the same intervals of contact being used, and the resulting deflections noted—two plates of each substance of the same thickness being used.

The results of the measurements are given in the following Tables, the effect of the glass being called 100.

TABLE I.

(*a*).

April 12, 1880.
Charge of battery, 2 sparks.
Contact 30 seconds.

Glass (1st plate)	100·0
Quartz (1st plate)	17·1
„ (2nd plate)	20·0
Calcite (1st plate)	0·0
„ (2nd plate)	0·0

(*b*).

April 13, 1880.
Charge of battery, 3 sparks.
Contact 20 seconds.

Glass (1st plate)	100·0
Quartz (1st plate)	19·3
Calcite (1st plate)	0·0

(*c*).

April 14, 1880.
Charge 3 sparks.
Contact 10 seconds.
Plates carefully dried by being in desiccator over night.

Glass (1st plate)	100·0
Quartz (1st plate)	10·7
Calcite (1st plate)	0·0

(*d*).

April 22, 1880.
Charge 2 sparks.
Contact 30 seconds.
Plate in desiccator since April 14.

Glass (2nd plate)	100·0
„ (1st plate)	96·3
Quartz (1st plate)	13·4
„ (2nd plate)	12·1
Calcite (1st plate)	0·0
„ (2nd plate)	0·0

TABLE II.

May 1. Relative Effects for different Intensities of Charge and Time of Contact.

Charge of battery.	Material.	Deflections, in millimetres.		
		Contact 5 seconds.	Contact 10 seconds.	Contact 20 seconds.
One spark	Glass (1st)	133·0	189·3	225·0
	Quartz (1st)	13·0	22·7	34·3
	Calcite (1st)	0·0	0·0	0·0
Two sparks	Glass (1st)	Off the scale	Off the scale	Off the scale
	Quartz (1st)	24·0	35·0	50·0
	Calcite (1st)	0·0	0·0	0·0

These Tables seem to prove beyond question that calcite in clear crystal has no electric absorption. Quartz seems to have about $\frac{1}{9}$ that of glass; but we have remarked that quartz is not a good substance to test the theory upon.

Some experiments were made with cleavage-plates of selenite, which are always more or less imperfect, as the laminæ are very apt to separate. These gave, however, effects about $\frac{1}{3}$ or $\frac{1}{4}$ those of glass.

In order to test still further the absence of electric absorption in calcite, the electrometer was rendered very sensitive, and the calcite plates were tested with gradually increasing

charges, from that which in glass gave 200 millim. after 1 second contact, up to the maximum charge (ten sparks of the unit-jar) which the condensers were capable of carrying. In these trials, the calcite still showed no effect, even with 30 seconds contact. During these experiments glass was frequently substituted for the calcite, to leave no question but that the apparatus was in working order.

It is to be noted that the relative effects of the quartz and the glass were different for dried plates and plates exposed to the atmosphere. This was possibly due to the glass being a better insulator, and thus retaining its charge better when dry than in its ordinary condition.

IV.

Thus we have found, for the first time, a solid which has no electric absorption; and it is a body which, above all others, the theory of Clausius and Maxwell would indicate. The small amount of the effect in quartz and selenite also confirms the theory, provided that we can show that in the given piece of quartz some molecules of right-handed quartz were mixed with the left; for we know that the theoretical conditions for the absence of electric absorption are rarely satisfied by laminated substances like selenite or mica. If the theory is confirmed, the apparatus here described should give the only test we yet have of the perfect homogeneity of insulating bodies; for any optical test cannot penetrate, as this does, to the very structure of the molecule.

XXIV. *On the Beats of Consonances of the Form* h : 1.

By R. H. M. Bosanquet.

[Plates VII.–X.]

Ohm's Law, and the Hypothesis of Resultant Displacements.

1. The doctrine known as Ohm's law states that the simplest form of motion by which definite musical pitch is defined to the ear is the pendulum-vibration. It may be extended as follows:—In all cases in which Ohm's law holds, the ear resolves any complex of two or more simultaneous pendulum-vibra-

tions into the original pendulum-vibrations of which they consist, and hears them as distinct and independent sounds.

I rest my belief in Ohm's law mainly on ordinary phenomena, not on refinements or difficult observations; and I shall endeavour to make this my course throughout.

2. So long as, in our mechanical arrangements, we approximate more and more nearly to the condition of things by which we know that simple harmonic vibrations must be produced, we also approximate in the character of the resulting sound to a pure and simple note of definite pitch, free from harmonics and other accompaniments.

So far as simple sounds are concerned, therefore, we receive Ohm's law as being at all events approximately true generally, and in all probability absolutely true when sounds of small intensity alone are considered.

3. When two different sounds are heard together, we have phenomena of which the following is a slight sketch.

If the two sounds are very nearly of the same pitch, they are not heard according to Ohm's law, *i. e.* separately and independently, but in the form of resultant displacements. The most important case is that in which the two sounds are of nearly equal intensity. In this case one sound is heard, intermediate in pitch between the two primaries, and oscillating in intensity between a certain maximum and nothing. These oscillations are what are called the beats of imperfect unisons. Now, as the two notes separate from one another in pitch, the character of the phenomenon changes; and at a certain point the two notes begin to be heard separately and independently, beside the beats which accompany them. It is this phenomenon that is accounted for by Helmholtz's theory of the existence of vibrating bodies, in the ear, having sympathy of a certain definite degree with the various notes.

4. Helmholtz ascertained the approximate degree of this sympathy by considerations of a somewhat indirect character. I wish to point out the important bearing, on the theory, of the direct determination of the interval which separates the region in which two notes are heard only as resultant displacements, from that in which they begin to be heard separately, in accordance with Ohm's law.

5. The experiments I have made on this point have been

mostly conducted by means of my enharmonic organ, in which I have a collection of notes separated for the most part by single commas.

The results, so far as I have gone, are:—1. The critical interval, at which two notes begin to be heard beside their beats, or resultant displacements, is about two commas, throughout that medium portion of the scale which is used in practical music. 2. This critical interval appears not to be exactly the same for all ears. In my own case two notes two commas apart are not heard distinctly beside the beats. In the case of Mr. Parratt, who has kindly examined the point with me, two notes two commas apart are distinctly heard beside the beats. In both cases the beats alone are heard with an interval of one comma, and the two notes are quite clear beside the beats with an interval of three commas.

I propose to undertake further experiments, with the view of determining this initial interval more accurately. So far as the above results go, they are quite consistent with Helmholtz's assumption as to the degree of sympathy of the ear.

6. Independently of any theory, the fact that at a certain point the ear begins to separate out two independent pendulum-vibrations from the resultant displacements, is one that must be recognized. It is easy to show that it is inconsistent with that representation of facts which assumes that beats arise only from the resultant forms exhibited by the superposition of the two vibrations on one receptive mechanism. This is shown as follows.

7. If we combine two vibrations of equal amplitude, which we may take $=1$, $\cos pt$ and $\cos(qt-\epsilon)$, on the same receptive mechanism, the effect is to produce a resultant displacement represented by

$$2\cos\frac{(p+q)t-\epsilon}{2}\cdot\cos\frac{(p-q)t+\epsilon}{2}.$$

This would be heard, by a hypothetical ear receiving the whole disturbance on the same sensorium, as a note whose frequency is the arithmetic mean between the frequencies of the two primaries, and having oscillations of intensity whose frequency is defined by a pendulum-vibration of frequency equal to half the difference of the frequencies of the primaries. This

is what is actually heard in the case of two notes less than two commas apart.

8. When the interval is greater than two commas, this ceases to represent the whole phenomenon perceived by the ear as it exists. The separate notes step in beside the resultant form represented by the above expression, with its beats and note having the frequency of the arithmetic mean. As the interval increases, the separate notes become more and more prominent, and the beats diminish in loudness and distinctness, till, by the time that a certain interval is reached, which is about a minor third in the middle of the scale, the beats practically disappear and the two notes alone survive.

9. It has been supposed by some that the beats disappear only in consequence of their rapidity *; and it is clear that under this supposition, as ordinarily made, lies the assumption that the mass of tone continues to be received in the same manner all the time—*i. e.* that the phenomena of the beats of imperfect consonances and combination-tones are to be explained by reasoning analogous to that of the above formula, which supposes the whole displacement reduced to its resultant on one receptive mechanism. This, for instance, is assumed whenever Smith's or Young's theories of beats are admitted as sufficient explanations of the phenomena.

10. In such cases, (*a*) it is forgotten that the fundamental assumption carries another consequence with it than those it was desired to explain; (*b*) the explanation itself also fails in an important point.

(*a*) The other consequence is, that if it were true that the receptive mechanism of the ear received a resultant displacement, so that the combination was as represented by the above formula, then the primary notes would not be heard at all, and the note that would be heard would have the arithmetic mean of the frequencies of the primaries.

E. g., in the case of a fifth (4 : 6) the note heard would be the major third (5), which would beat very rapidly; just as, when I myself hear the resultant of notes two commas apart, it is one note midway between them beating rapidly. But, as a matter of fact, the note 5 is not heard at all in the above case.

* This is absolutely disproved by the argument in Helmholtz's *Tonempf.* p. 286, ed. 4.

(b) Again, supposing that in some unexplained way the beats whose speed is $\frac{p-q}{2}$ in the above notation gave rise to a note, as supposed by König. Then the speed of that note does not agree with that required for König's first beat-note, which has the same speed as Helmholtz's difference-tone, or $(p-q)$ in the above notation.

11. The relationship of these resultant displacements to the phenomena in the general case, is most conveniently studied by means of the curves drawn by Donkin's harmonograph. The instrument in my possession has a rather restricted number of change-wheels; and one of my first tasks in the St. John's College laboratory has been to cut additional change-wheels for this instrument, for the purpose of illustrating this subject graphically*. (See Plates VII. to X.)

12. The examination of the curves leads us to the following conclusions.

In every case, whether of beats of unisons, or of beats of imperfect consonances, the examination of the curves shows a portion of a harmonic curve lying through the vertices of the single resultant vibrations, which portion corresponds in duration to the beats as given either by Smith's rule or the ordinary rule for beats.

The durations of these harmonic curves are different in different cases. Three principal types may be distinguished:—

Let E, F be the amplitudes; $p : q$ the ratio in lowest terms of the exact consonance whose small variation is considered $(q > p)$.

(1) If $E \mid p$ is considerably less than $F \mid q$, there are q complete harmonic curves both at top and bottom, and the duration of each is q times that of the Smith's beat.

(2) If $E \mid p = F \mid q$, there are $p+q$ complete harmonic curves which may be called external, passing both top and bottom, and the duration of each external curve is $p+q$ times that of the Smith's beat; also there are $q-p$ internal curves, which lie nearer the middle; the duration of each internal curve is $q-p$ times that of the Smith's beat.

(3) If $E \mid p$ is considerably greater than $F \mid q$, there are p

* These curves are of such interest that I devote some space to their discussion, § 77 &c.

harmonic curves both at top and bottom. They are not complete, but appear to form portions of curves of long period.

13. In all cases the curves which correspond to the beats, as ascertained by Smith's method or the ordinary formula, lie like series of bows, one series at the top and the other at the bottom.

The complete period of the pendulum-vibration, of which each of these bows forms a part, is always longer than the single bow or Smith's beat, according to the above rules.

14. Now, according to a well-known principle of mechanics, no pendulum-vibration can give rise to one of another period, in a system in which the forces are proportional to the displacements.

15. In the present case, if we find a term present whose whole period is that of the Smith's beat, it must therefore arise by transformation, *i. e.* through the presence of terms of higher orders than the first. We shall use generally the expression "transformation" to signify the effect on a system of terms higher than the first in the expression for the forces. This is substantially Helmholtz's explanation of the difference-tone, which is identical with the lowest beat-note of König.

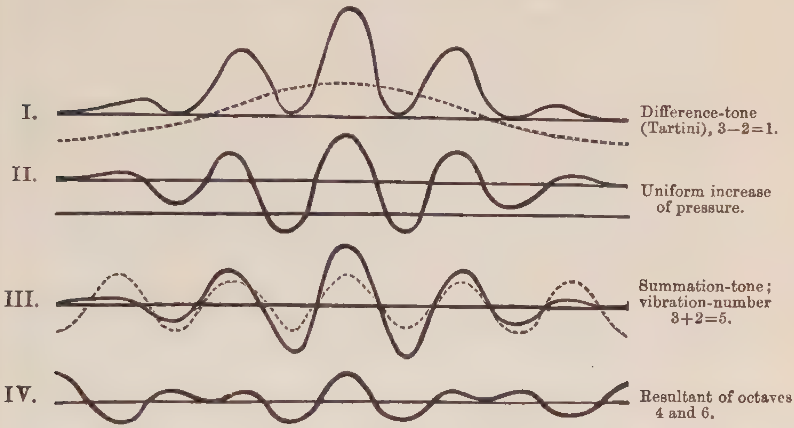
16. We shall show that all König's beat-notes can be accounted for in a similar manner, by the assumption that terms of higher orders become important in the mechanism of the ear when the displacements are considerable.

17. We can illustrate further the difference produced in the curves by the admission of the difference-tone or beat-note as part of the mass of sound. The characteristic difference is, that the medial line is itself bent into a curve, whose whole period is that of the Smith's beat. I have not been able to draw any long curve to show this; but the appearance of the curve at the top of illustration A (p. 227) is very like it in a general way. This illustration represents the square term of the force developed by a fifth (2 : 3) in a transforming system. B is the figure of the total disturbed force in a similar case; but the throwing-up of the medial line is not so prominent as it would be in a longer curve. I have, however, no machine that will draw the combination; and the construction of a long curve of this kind is not worth the labour it would entail.

18. We sum up this part in the following conclusions:—

A.

Statical Analysis of Disturbance proportional to Square of Displacement, showing origin of the combination-tone. Fifth 2 : 3.



I. From disturbance proportional to square of displacement subtract simple e. Amplitude $\frac{1}{4}$ of disturbance. Period = that of the beat. (Vibration-number = difference of vibration-numbers, 3—2=1.)

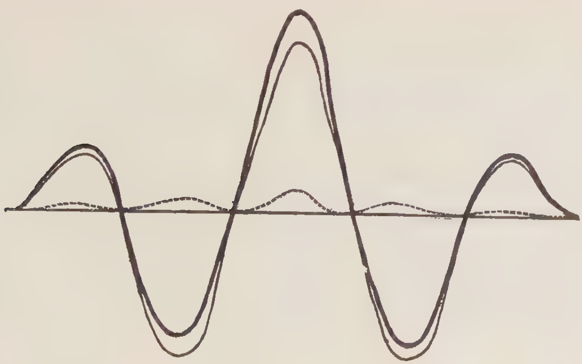
II. Subtract $\frac{1}{4}$ of amplitude of disturbance.

III. Subtract a simple tone. Amplitude $\frac{1}{4}$ of that of disturbance. Period = $\frac{1}{2}$ that of resultant vibration in original beat. (Vibration-number = sum of vibration-numbers of original tones, 3+2=5.)

IV. There remains a succession of beats like the original beats of the fifth, an octave higher, *i. e.* the combination of the octaves of the original notes.

B.

Beat of Fifth, 2 : 3. *Disturbed by term proportional to square of displacement, showing origin of the combination-tone.*



As two notes of equal amplitudes separate from unison, they are at first received by the ear in the manner of resultant displacements, consisting of the beats of a note whose frequency is midway between that of the primaries.

When the interval reaches about two commas, the ear begins to resolve the resultant displacements, and the primary notes step in beside the beats.

When the interval reaches a minor third in the ordinary parts of the scale, neither the beats nor the intermediate pitch of the resultant note are any longer audible, at least as matter of ordinary perception; but the resultant displacement which reaches the ear is decomposed, and produces the sensation of the two primary notes, perfectly distinct from each other: that is to say, Ohm's law has set in, and is true, for ordinary perceptions and in the ordinary regions of the scale, for the minor third and all greater intervals.

19. We may notice here incidentally that it is necessary that the resolved primaries should be uniform and steady, in order that the beats exhibited in the resultant forms may retain their regularity. Those who support the Young-and-Smith theory generally have a sort of confused idea that the primaries are modified when superposed into their resultant.

20. How, then, do the beats of mistuned consonances arise? They may be regarded as springing from interference of new notes, which arise by transformation, in the passage of the resultant forms through the transmitting mechanism of the ear, before the analysis by the sensorium.

Experiments.

21. The engine and bellows* being adjusted to run continuously and quietly, I began to follow the course of König's experiments at the point where he deals with the combinations of the note C, following his form not accurately, but with such divergences as the difference in the apparatus suggested. After going through one or two sets in the way hereinafter described, I concentrated my attention on the analysis of beats, and specially on those of mistuned consonances of the form $h:1$. It will be seen that after a time I entirely discarded resonators, having convinced myself that, so far as

* See Phil. Mag. Oct. 1880.

they were concerned, the beats of mistuned consonances, other than unisons, with the beat-notes, difference- and combination-tones of all orders, and, in fact, all that I had to observe, were of a purely subjective nature, and were extinguished by resonators properly used, so far as my arrangements enabled me to perceive.

22. The mode in which I then pursued the observations on the beats of the mistuned consonances in question was, to adjust the notes and leave them sounding uniformly and continuously by the hour together. I then walked about the room listening to the combination in all the various forms in which it presented itself, went outside and came in again, always keeping in view the question, what are the sounds that these beats consist of?

23. It is hard to believe that a question to which the answer is tolerably simple could be so difficult. Yet it is *very* difficult; it is one of the most difficult things I ever tried to do, to analyze these apparently complicated sounds into their elements by the ear alone. And when I state my results, I must not be taken to mean that the elements I mention are all that are present. In fact, one of the great difficulties is that there appear to be such a number of different sounds. Some of these are probably due to the imagination; others probably exist in small intensity. And I am satisfied that there still exists a large field of work in the further prosecution of this subject. But of the main result I have no doubt whatever; and that is:—

24. The beats of mistuned consonances of the form $h:1$, where h is nearly some whole number, consist mainly of variations of intensity of the lower note when the beats of the harmonics are eliminated.

25. I was prepared for this result in the case of the octave by my preliminary experiments (Phil. Mag. viii. p. 293), but did not proceed further till I had verified it and got my ear to perceive it readily under the new conditions, which required two or three days. I then got Mr. Parratt to come and listen. He was much disturbed by the trifling noises from the engine, belts, &c.; and I blew the bellows myself for a time. Eventually he came to the same conclusion, but with an amount of hesitation and difficulty which showed

me what an important element practice is in these observations.

26. I then started these observations with the mistuned twelfth, proceeding in the same way. I seemed to have the same difficulty as before in seizing the phenomenon; and when I eventually decided that these beats were also on the lower note, it was not in pursuance of any preconceived conclusion; for I had no idea at that time of the explanation I now give, and certainly none of the presence of the second difference-tone, or its identity with one of König's beat-notes.

27. Having got so far, I found the remaining verification, of the beats of the mistuned double octave, somewhat easier. These are also on the lower note when they are heard. I have never heard the beats of a mistuned consonance with any wider interval, with the notes I employ, as clear and unmistakable phenomena. Such beats may be discernible by more acute ears, or with notes of a more powerful quality, as they were discerned by König. But in such cases it will be incumbent on the observer to purge the beats from the suspicion of containing the beats of harmonics, as I have done.

28. Mr. Parratt subsequently convinced himself, as before, that the beats of the twelfth and double octave were all heard on the lower notes. I endeavoured, as far as possible, to make his observations independent by avoiding communicating my conclusions to him beforehand.

29. The elimination of the beats of the harmonics depends on the following considerations. The notes employed were examined, with and without resonators, as to the presence of harmonics. These, so far as they are objective, are readily detected with resonators. The beats of the harmonics, where they existed objectively, were also examined with resonators. After a little practice the sound of these beats became familiar enough to prevent their being confused with the beats of the low notes, and the two sets of beats could be observed independently.

30. The only harmonic that exists in these notes in sensible intensity is the twelfth; and this does not appear to originate in the same manner as the principal note. It is heard separately, as it were, and as if it had an independent origin. It

seems probable that it arises in connexion with the movements of the air about the mouth-piece, and not by resonance in the cavity of the bottle, like the principal note. At all events, whatever the cause may be, the effect is that the presence of this note is readily distinguished and allowed for, and there is no risk of its being mixed up with the rest of the phenomenon.

31. The notes employed are of moderate strength. It seems to me that the employment of notes of great power is open to the objection that it introduces all sorts of transformations depending on the greatness of the diplacements; and in this respect alone König's procedure is open to considerable objection. I have confined myself to notes of moderate strength, lying in those regions of the scale which are in ordinary use in music. It is phenomena thus presented that we really seek to understand; and I do not think that any thing is gained by pushing the investigation into those extreme regions where it is possible and highly probable that the ordinary laws of hearing become modified.

32. The first series of notes examined in the above manner were the set of pairs

$$\begin{array}{c} C : c \\ C : g \\ C : c' \\ \hline C : e' \\ C : g' \\ C : c'' \end{array}$$

The beats produced by mistuning, when cleared of the harmonic beats, were heard only in the first three cases.

The second set of pairs was

$$\begin{array}{c} c : c' \\ c : g' \\ \hline c : c'' \\ c : e'' \\ c : g'' \end{array}$$

The beats in question were only heard in the first two cases.

The third set was

$$\begin{array}{c} c' : c'' \\ \hline c' : g'' \\ c' : c''' \end{array}$$

The beats in question were only heard in the first case.

33. In the few experiments hitherto made with notes of higher pitch, the beats of mistuned consonances of the form $h:1$ were not heard when the beats of the harmonics were eliminated, unless the power of the notes was very greatly increased. In this region, however, König's own observations are very full and complete.

34. We notice at once the decrease in the range within which the phenomena are heard as we rise in the scale. This is at once accounted for on the hypothesis of transformation, by the consideration that the displacements to which the higher notes give rise are much smaller than those of the lower notes. If we knew the law of the decrease, we might obtain a relation between the coefficients of the different terms in the expression for the character of the transforming mechanism. König has attempted to formulate a law of decrease; and I have done so on a previous occasion; but this part of the subject is as yet too hypothetical to admit of satisfactory treatment.

Objective and Subjective Phenomena.

Resonators.

35. On beginning work I endeavoured, in the first instance, to ascertain what evidence resonators are capabable of furnishing as to the nature of binary combinations. There are a few points connected with their use which require attention.

36. I have always found difficulty in getting results of a definite character with resonators, whether applied directly to one ear in the manner described by Helmholtz, or connected with one ear by means of a flexible tube, as practised by others. There are three difficulties which occur: (1) pressing the tube or orifice into the ear is apt to close the inner passage of that organ; (2) if the tube or orifice is applied lightly, it does not completely occupy the passage, and external sound comes past it into the ear; and (3) it is impossible so to stop the unused ear as to prevent the external impressions from arriving there and causing confusion.

37. The method I ultimately adopted was as follows:—A copper tube of $\frac{1}{4}$ inch diameter was bent into a semicircle, the diameter of which was nearly 8 inches. At the middle of the tube, and at right angles to its plane, another copper tube was

inserted, 2 inches long, which tapered down to an orifice $\frac{1}{8}$ inch in diameter; this served to communicate with the interior of a resonator by means of a small flexible tube. The extremities of the semicircle were turned inwards and upwards; and into them two brass tubes were inserted, $\frac{3}{4}$ inch long and $\frac{1}{8}$ inch in internal diameter, screwed on the outside. Over each of these was fitted a brass tube, screwed inside, carrying an ivory nipple, such as is used for ear-trumpets. I generally covered the nipple with a couple of thicknesses of thin india-rubber tube.

38. When used, the semicircle is passed under the chin with the resonator-attachment projecting in front. The nipples are at first screwed back as far as possible, brought opposite the orifices of the ears, and then screwed forward until they enter the ears. They are then gradually advanced until the passages are closed to external sounds. Something depends on the way the tube is held. With practice it is possible to hold it so that the passages are closed to external sounds without screwing the nipples in very tight. When they are screwed very tight, it is rather unpleasant, and even painful. But it is necessary constantly to be on one's guard against being deceived by an occasional entrance of external sounds if the nipples are not quite tight. This instrument was made for me some time ago by Mr. Walters of Moorgate Street; it has already been described (Proc. Mus. Assoc. 1879–80, p. 18).

39. The resonators I employ are bottles fitted with corks having apertures of various sizes. I sometimes tune them with water, in the same way as the bottle-notes; sometimes I insert tubes into the apertures to lower the pitch. A bit of small glass tubing passed through the cork is connected by an india-rubber tube with the above-described ear-piece.

40. By means of these arrangements I some time ago examined the nature of the ordinary first difference-tone, and convinced myself that it is not capable of exciting a resonator (*l. c.* p. 20). This conclusion has also been arrived at by others*. In short, the difference-tone of Helmholtz, or first beat-note of König, as ordinarily heard, is not objective in its character. It is therefore subjective. (See Helmholtz, *Ton-*

* Preyer, *Akustische Untersuchungen*, p. 13.

empfindungen, 4th ed. p. 259.) In making the experiment of listening for the difference-tone through a resonator, it is necessary to be careful that the ears are both closed to external sounds; otherwise the external notes will penetrate through, the difference-tone will appear, and the completeness of the cut-off effected by the resonator will be entirely lost.

41. When beginning the regular course of experiments according to the general outline of König's work, I was careful, in the first instance, to examine the various masses of sound presented, with resonators arranged as above indicated. In examining, for instance, the intervals made by the note C with the various notes of the octave above it (up to *c*), I first fixed the resonator at some one pitch, and then ran the movable note up through the octave. Then, as this did not seem a good process for analysis, I set the mistuned octave beating, or any other combination it was desired to examine, and ran the pitch of the resonator up and down with water to see if any thing could be detected. And here I came across an observation that puzzled me for some time.

42. Suppose the mistuned octave C : *c* was sounding, and I examined the lower note with the resonator: sometimes it appeared loud and steady, at other times as if beating powerfully. On removing the resonator-attachment from the ear, the lower note was always heard to beat powerfully. The explanation was simple. When the nipples of the resonator-attachment fitted tightly into the ears, nothing reached the ear but the uniform vibrations of the resonator sounding C. But if there was the slightest looseness between the nipple and the passage of either ear, the second note (*c*) of the combination got in, and gave rise to the subjective difference-tone (first beat-note of König), by interference of which with the C I explain the beats on that note. *These beats are therefore subjective.*

43. A considerable number of combinations, including examples of the principal forms of beat, rattle, or roll, were examined in this way; and when the precautions above indicated were attended to, the results were in all cases to negative the objective existence of all forms of beats, and beat-notes or difference-tones, except the beats which arise from the interference of approximate unisons, which beats arise from

both notes acting on the resonator simultaneously. This of course includes the beats produced by objective harmonics.

Course of General Experiments.

44. The following is the detailed examination of the combinations of the note C, made in a continuous and connected manner. The results have a general correspondence with those of König. The numerous rattles and rolls of beats mentioned were not further analyzed for the most part: the analysis of these is very difficult; and, as has been already stated, a separate investigation is required in every such case. Some attention was devoted to beats of the mistuned fifth, both in the case mentioned and in others; but no final result was arrived at. In two different cases of mistuned fifths (2 : 3, nearly), I had a strong impression that the note 7 formed an important part of the beat. This would be a summation-tone of the second order, thus $2 \times 2 + 3$. I am confident that it did not arise from harmonics.

These experiments were made after some experience had been gained.

C : C.

Rattle up to

C : F.

Slow beats up to

C : G, smooth fifth. Roll only perceptible when the ear is held close to the two sources of sound.

——, 5 beats sharp. Perception of pitch very difficult in this part of the scale. There is a heavy beat like a knock, which appears to affect the whole mass of sound*. The low beat of C_1 is only distinguishable with difficulty, or hardly at all.

(Another occasion.) Mr. Parratt describes the fifth C : G, beating slowly, as consisting of E♭ and C_1 in addition to the primary notes; the mass of the beat is at least partly on E♭. I do not hear the E♭, but seem to hear the note E.

(Another occasion.) Mr. Parratt is clear that the beat of the mistuned fifth C : G is on C_1 alone; but he still

* I take this entry to show that no progress had been made with the resolution of the phenomenon into its elements.

hears the E♭ in the mass of tone. I seem to hear the beats both on C and C_1; but I have a difficulty in separating the octaves in this deep pitch.

C : G, 8 beats sharp. Clear rattle, with suspicion of roll beside it.

—— 10 beats sharp. Beats just distinguishable. Roll.

C : B♭. Rattle emerges.

Below

C : *c*, 8 beats can be counted.

——, 4 beats very distinct. Consist entirely of variation of intensity of lower note. This effect is very clear and remarkable.

——, a very slow beat flat. Here it was easy to recognize the effect of the shift of phase in the apparent great increase of volume of the lower note at one period of the change. The upper note was not perceptibly affected.

C : *c*. A slight rich roll with smooth tone. The production of the roll depends a good deal on the phase, as is seen by leading up to *c* with a very slow beat.

The twelfth of the C was plainly distinguishable, but it appeared to keep separate from the mass of tone; it was perfectly steady and unaffected by combination with *c*.

C : *c*, 2 beats sharp. Phenomena undistinguishable from 2 beats below.

——, 4 beats sharp. Perhaps a little less roll in the strong part of the beat.

——, 8 beats sharp. The mass of the beats is of pitch near C; but the exact pitch is very difficult to distinguish. It is a deep heavy rattle, quite distinct in pitch from the upper note.

C : *e*. If there is any slow beat in passing through this, it is very difficult to distinguish. I am inclined to negative it.

C : *f*♯. Roll.

Slow beats up to

C : *g*. These beats consist of alternations of intensity of C. They are more difficult to count than those of C : *c*. I counted them at 5 below.

——. Slow beats above.

C : *b*♭. Rattle, turning into beats easily counted at 4 below

c'. These beats also consist of variations of intensity of the lower note.

C : c'.

The beats above c' were also counted at 4 above, while the engine was going, without difficulty.

45. Above this, in the neighbourhood of the binary consonances C : e' &c., I have never been able to obtain slow beats in such a way that they could be readily perceived (even without the engine) or certainly counted.

46. The mode adopted to examine cases in which the beats could not be perceived was, to introduce a third note, such as c', which gave beats with the C, and tune it true. Then any note, such as e' or g', could be readily tuned so that the whole three notes gave 1, 2, 3, or 4 beats. When this had been done, the intermediate note c' was removed. If the pair examined was capable of giving beats at all, they should then have been audible.

47. The details of the above course furnish no new results; I have not, therefore, thought it worth while to give similar courses for other sets of notes. Those results which are worthy of mention have been already stated.

Theory of the Beats of Mistuned Consonances of the form h : 1.

48. Let n be the frequency of the lowest note, m the number of beats per second. Then the mistuned octave is $n : 2n \pm m$; the mistuned twelfth is $n : 3n \pm m$; and so on.

49. Beats of the mistuned octave,

$$n : 2n \pm m.$$

Number of beats $= m$.

m variations of intensity of the lower note (n) are produced by interference of notes n and $n \pm m$; and $n \pm m$ is the first combination-tone (difference-tone of form $p-q$), of the primaries n and $2n \pm m$.

This rests chiefly on the observation that the beats, when the octave harmonic is eliminated, consist entirely of variations of intensity of the lower note.

The existence of the first combination-tone in question $(p-q)$ is well known. It is easily demonstrated in the neighbouring case of intervals not far removed from the fifth, when

the beats of the first two combination-tones are specially prominent (secondary beats of König).

50. Beats of the mistuned twelfth,

$$n : 3n \pm m,$$

Number of beats $= m$.

m variations of intensity of the lower note (n) are produced by interference of notes n and $n \pm m$. And $n \pm m$ is the second combination-tone (difference-tone of form $2p - q$) of the primaries n and $3n \pm m$.

This rests also chiefly on the observation that the beats, when the third partials are eliminated, consist entirely of variations of intensity of the lower note.

The existence of the second combination-tone in question ($2p - q$) is demonstrated in many cases by König. It is easily heard in the case of intervals near the octave high in the scale. It is also easily detected by the secondary beats which it forms with the first combination-tone in the case of intervals near the fifth—also less easily by the secondary beats which it forms with the third combination-tone in intervals near 2 : 5, at which point the second and third combination-tones coincide.

51. Beats of the mistuned fifteenth or double octave,

$$n : 4n \pm m.$$

Number of beats $= m$.

m variations of intensity of the lower note (n) are produced by interference of notes n and $n \pm m$. And $n \pm m$ is the third combination-tone (difference-tone of form $3p - q$) of the primaries n and $4n \pm m$.

This rests also chiefly on the observation that the beats, when the fourth partials are eliminated, consist entirely of variations of intensity of the lower note.

The existence of the third combination-tone in question ($3p - q$) is demonstrated in many cases by König. It is heard not so easily as the lower combination-tones, in the case of intervals near the twelfth high in the scale. It is also less easily detected by the secondary beats which it forms with the second combination-tone in the case of intervals near 2 : 5, at which point the second and third combination-tones coincide—also much less easily by the secondary beats which it forms with the fourth combination-tone in the case of inter-

vals near 2 : 7, at which point the third and fourth combination-tones coincide.

52. Beats of the mistuned tierce (two octaves and a major third),

$$n : 5n \pm m.$$

These beats are much less easily detected in pure notes of the ordinary strength than any of the foregoing. They are recorded by König; but I have never heard them clearly. As it is certain that König's notes were not perfectly pure, and he does not analyze the beats, we cannot tell whether the variations of the lower note were produced in his experiments. If they were, they are to be accounted for in a similar manner.

Number of beats $= m$.

m variations of intensity of lower note (n) are produced by interference of notes n and $n \pm m$. And $n \pm m$ is the fourth combination-tone (difference-tone of form $4p - q$) of the primaries n and $5n \pm m$.

The existence of the fourth combination-tone in question ($4p - q$) is demonstrated directly by König in the case of intervals near the double octave $c''' : c^{v}$. It is also less easily detected by the secondary beats which it forms with the third combination-tone in the case of intervals near 2 : 7, at which point the third and fourth combination-tones coincide.

53. Beats of the mistuned consonance of the nineteenth are recorded by König;

$$n : 6n \pm m.$$

Number of beats $= m$.

m variations of intensity of lower note (n) might be produced by interference of n and $n \pm m$. And $n \pm m$ is the fifth combination-tone (difference-tone of form $5p - q$) of the primaries n and $6n \pm m$.

The existence of the fifth combination-tone in question ($5p - q$) is not anywhere directly demonstrated. Secondary beats, which might be produced by its interference with the fourth combination-tone, are recorded by König in the neighbourhood of the interval $c : d''$

54. Beats of the mistuned consonance 1 : 7 are recorded by König. These might be produced by a sixth combination-tone (difference-tone of form $6p - q$) of the primaries n and $7n \pm m$.

55. Beats of the mistuned consonance 1 : 8 are recorded. These might be produced by a seventh combination-tone (difference-tone of form $7p-q$) of the primaries $8n \pm m$.

56. As far as my own experience goes, however, I have no direct and palpable evidence of beats of mistuned consonances higher than 1 : 4, or of the existence of combination-tones higher than the third $(3p-q)$ in recognizable intensity. Up to this point the phenomena are quite clear; and there is no possible doubt as to their nature.

But in considering these limited results it must be remembered, (1) that I have restricted myself to notes of very moderate intensity, so that the phenomena might correspond as nearly as possible to those which are presented to our ears in practice, and (2) that, although I was unable to get rid entirely of the presence of upper partials in all cases, yet the phenomena were subjected to a careful and prolonged analysis by listening under varied conditions, until the effect of the upper partials could be separated out and eliminated with certainty. And we have at all events no security that these upper partials did not give rise to many of König's results; indeed it is almost certain that they must have entered into those results.

Note.—The present paper was written before the appearance of König's paper in Wiedemann's *Annalen* in the present year. The discussion of that paper, though necessary for a complete view of the subject, must be reserved till after the conclusion of the present paper.

Combination-Tones arising from Terms of Orders higher than the first, in the Transforming-structure of the Ear.

57. Helmholtz pointed out the way in which the hypothesis of asymmetry in the transmitting mechanism of the ear gives rise to the combination-tone of the first order, which he called the difference-tone. This asymmetry was represented in his investigation by a term of the second order in the force called into play by a given displacement. Helmholtz further indicated the tones to which the existence of terms of the third order gives rise, and pointed out that tones of the fourth order &c. would give rise to other combination-tones not further specified.

58. In specifying those combination-tones which arise from

the terms of the third order, Helmholtz pointed out that one of them was a combination-tone of the second order "in the sense indicated by Hallström." This is also the sense in which the expression "combination-tone of the second order" is used by Helmholtz himself in the text of his work; and it means apparently a combination-tone which arises from a combination-tone or combination-tones of the first order when combined with any other notes present, or with each other, according to the law of combination-tones of the first order.

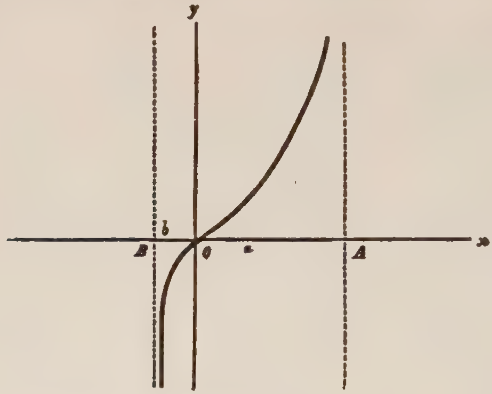
59. It is clear, however, that the principal combination-tones which arise from the terms of higher orders in the transmitting mechanism of the ear, are derived directly from the primary tones, and are not materially influenced by the secondary series of tones. This is obvious, on the one hand, since all the resultant tones, according to the principles of their origin, are of the nature of small quantities compared with the primary tones, and, on the other hand, because the tones derived from the terms of higher orders are in fact produced with the greatest intensity when the tones derived from terms of lower orders are weak or evanescent. This fact has been used by König as one of the most powerful objections to the theory of combination-tones as hitherto expounded; and, indeed, the objectionable character of some of the hypothetical derivations by combination given by Helmholtz* must have struck many readers independently.

60. I shall now examine Helmholtz's hypothesis of asymmetry in a little more detail; and I think it will appear that it leads, by tolerably simple mathematical treatment, to the development of the combination-tones of the higher orders, under the circumstances under which they actually exist, and independently of the combination-tones of the lower orders.

61. Let 0 represent the position of rest of a point free to move along the line $0x$ between the points A and B, subject to certain forces in that line. Suppose that these forces are of the nature of springs tending to resist the departure from 0, and that on arrival at the points A, B, at distances a, b, from 0 on either side, the springs ultimately go up against dead walls, so that further displacement is resisted with an infinite

* *Tonempfindungen*, 4th ed. p. 329; also p. 327, where the combinations are supposed to be formed with the partials of the primaries as well.

force. If we set off the forces as ordinates, they may be represented as in the figure; and analytically they may be ex-



pressed by such an assumption as

$$y = \frac{kx}{\left(1-\frac{x}{a}\right)\left(1+\frac{x}{b}\right)}$$

Expanding this function in a series proceeding by powers of x, we have an example of a law of force expressed by such a series, which for small displacements coincides with the pendulum law, while for displacements of but moderate extent the higher terms rapidly become prominent.

62. Assuming the existence of a law of this kind in the transmitting mechanism of the ear, we should have for the force corresponding to displacement u such an expression as

$$-(n^2u + \alpha u^2 + \beta u^3 + \gamma u^4 + \dots).$$

Suppose the system acted upon by two harmonic forces,

$$\mathrm{E}\cos pt, \quad \mathrm{F}\cos(qt-\epsilon),$$

and the mass $=$ unity, or included in the coefficients; the equation of motion is

$$\frac{d^2u}{dt^2} + n^2u = -\alpha u^2 - \beta u^3 - \gamma u^4 \dots + \mathrm{E}\cos pt\ \mathrm{F}\cos(qt-\epsilon)$$

(following the notation of Lord Rayleigh on Sound, i. p. 65).

For the first approximation we neglect powers of u above

the first; then

$$u = e\cos pt + f\cos(qt - \epsilon),$$

where

$$e = \frac{E}{n^2 - p^2}, \quad f = \frac{F}{n^2 - q^2}.$$

63. We may here mention that, in the present case, n is negligible. This is easily seen, since, if n had any value corresponding to a frequency within the limits of the ordinary range of hearing, there would be a series of notes strengthened by the correspondence. But the only notes thus strengthened are those which are supposed to correspond to the ear-cavity. They are so high in the scale that the connexions of the internal ear would require to be nearly as rigid as brass or steel to produce them. A further reason for n not being large will be arrived at in speaking of combination-tones. And we shall assume that n is smaller than any values of p or q which occur in practice.

64. The first approximations to the subsequent terms may be now all made by substituting in them the value of u above obtained. The process to be followed for u^2 coincides with that commonly adopted; and the result is given in Lord Rayleigh's book, i. p. 66.

In the cases of u^3 and higher powers the process is simpler than that which has been previously indicated.

65. In virtue, however, of the preceding considerations concerning the value of n, we may materially simplify the whole process for our present purpose by neglecting the term n^2u altogether. The original equation then assumes the form

$$\frac{d^2u}{dt^2} = -(\alpha u^2 + \beta u^3 + \dots) + E\cos pt + F\cos(qt - \epsilon).$$

First approximation,

$$\begin{aligned} u &= D^{-2}\big(E\cos pt + F\cos(qt - \epsilon)\big) \\ &= -\left(\frac{E}{p^2}\cos pt + \frac{F}{q^2}\cos(qt - \epsilon)\right) \\ &= -\big(e\cos pt + f\cos(qt - \epsilon)\big) \text{ say.} \end{aligned}$$

Substituting this in the remaining terms, we get

$$\frac{d^2u}{dt^2}=\mathrm{E}\cos pt+\mathrm{F}\cos(qt-\epsilon)$$
$$-\alpha\Big(e^2\cos^2 pt+f^2\cos^2(qt-\epsilon)+2ef\cos pt\cos(qt-\epsilon)\Big)$$
$$-\beta\Big(e^3\cos pt+f^3\cos^3(qt-\epsilon)+3e^2f\cos^2 pt\cos(qt-\epsilon)$$
$$+3ef^2\cos pt\cos^2(qt-\epsilon)\Big)$$
$$-\gamma\Big(e^4\cos^4 pt+f^4\cos^4(qt-\epsilon)+4e^3f\cos^3 pt\cos(qt-\epsilon)$$
$$+6e^2f^2\cos^2 pt\cos(qt-\epsilon)+4ef^3\cos pt\cos(qt-\epsilon)^3\Big)$$
$$-\quad.\quad.\quad.\quad.\quad.\quad.\quad.\quad.\quad;$$

and u is, to this first approximation with respect to all terms, the integral taken twice with respect to t of the right-hand side of the above equation.

66. There is, no doubt, a difficulty as to the absolute neglect of the term n^2u. The effect is to make the vibrating-point apparently rest in a position which is not one of equilibrium. Nevertheless the application of the facts to Helmholtz's hypothesis requires this proceeding; and it makes no difference whether it is done finally or at first. I think it very probable that damping terms, depending on the second and higher powers of the velocity, play an important part in the real explanation. The source of the terms, however, is of secondary importance in the present state of the question. The point is to show that those resultant sounds which depend on terms of higher orders can become great independently of those which depend on terms of lower orders.

67. Collecting the terms up to the fourth order, transforming them into multiple arcs, and writing $pt=\theta$, $qt-\epsilon=\phi$, the equation becomes

$$\frac{d^2u}{dt^2}=\mathrm{E}\cos\theta+\mathrm{F}\cos\phi$$
$$-\Big[\frac{\alpha}{2}(e^2+f^2)+\tfrac{3}{8}\gamma(e^4+f^4+4e^2f^2)$$
$$+\tfrac{3}{4}\beta\{(e^2+2f^2)e\cos\theta+(2e^2+f^2)f\cos\phi\}$$
$$+\frac{e^2}{2}\{\alpha+\gamma(e^2+3f^2)\}\cos 2\theta+\frac{f^2}{2}\{\alpha+\gamma(3e^2+f^2)\}\cos 2\phi$$
$$+\frac{\beta}{4}(e^3\cos 3\theta+f^3\cos 3\phi)$$

$$+\frac{\gamma}{8}(e^4 \cos 4\theta + f^4 \cos 4\phi)$$

$$+ef\{\alpha+\tfrac{3}{2}\gamma(e^2+f^2)\}(\cos(\theta+\phi)+\cos(\theta-\phi))$$

$$+\tfrac{3}{4}\beta e^2 f\{\cos(2\theta+\phi)+\cos(2\theta-\phi)\}$$

$$+\tfrac{3}{4}\beta e f^2\{\cos(\theta+2\phi)+\cos(\theta-2\phi)\}$$

$$+\tfrac{3}{4}\gamma e^2 f^2\{\cos 2(\theta+\phi)+\cos 2(\theta-\phi)\}$$

$$+\gamma\frac{e^3 f}{2}\{\cos(3\theta+\phi)+\cos(3\theta-\phi)\}$$

$$+\gamma\frac{ef^3}{2}\{\cos(\theta+3\phi)+\cos(\theta-3\phi)\}\Big].$$

68. On performing the double integration, we shall find the constant term in the above multiplied by t^2, an inadmissible result. It is only necessary to look back to the result of the complete process, when we find that the term in question is represented after integration by $\dfrac{\text{constant}}{n^2}$, where n^2 is the small coefficient of the term we have neglected. This indicates that the position of equilibrium is indeed displaced, but through a finite amount; as this does not affect our results, we omit the term in question.

69. Remembering that $\theta=pt$ and $\phi=qt-\epsilon$,

$$e=\frac{\mathrm{E}}{p^2},\quad f=\frac{\mathrm{F}}{q^2},$$

the remainder of the equation becomes

$$u=-e\cos\theta-f\cos\phi$$

$$+\tfrac{3}{4}\beta\left\{\frac{e^2+2f^2}{p^2}e\cos\theta+\frac{2e^2+f^2}{q^2}f\cos\phi\right\}$$

$$+\frac{e^2}{2}\frac{\alpha+\gamma(e^2+3f^2)}{4p^2}\cos 2\theta+\frac{f^2}{2}\frac{\alpha+\gamma(3e^2+f^2)}{4q^2}\cos 2\phi$$

$$+\frac{\beta}{4}\left(\frac{e^3}{9p^2}\cos 3\theta+\frac{f^3}{9q^2}\cos 3\phi\right)$$

$$+\frac{\gamma}{8}\left(\frac{e^4}{16p^2}\cos 4\theta+\frac{f^4}{16q^2}\cos 4\phi\right)$$

$$+ef\{\alpha+\tfrac{3}{2}\gamma(e^2+f^2)\}\left(\frac{\cos(\theta+\phi)}{(p+q)^2}+\frac{\cos(\theta-\phi)}{(p-q)^2}\right)$$

$$+\tfrac{3}{4}\beta e^2 f\left\{\frac{\cos(2\theta+\phi)}{(2p+q)^2}+\frac{\cos(2\theta-\phi)}{(2p-q)^2}\right\}$$
$$+\tfrac{3}{4}\beta e f^2\left\{\frac{\cos(\theta+2\phi)}{(p+2q)^2}+\frac{\cos(\theta-2\phi)}{(p-2q)^2}\right\}$$
$$+\tfrac{3}{4}\gamma e^2 f^2\left\{\frac{\cos 2(\theta+\phi)}{4(p+q)^2}+\frac{\cos 2(\theta-\phi)}{4(p-q)^2}\right\}$$
$$+\gamma\frac{e^3 f}{2}\left\{\frac{\cos(3\theta+\phi)}{(3p+q)^2}+\frac{\cos(3\theta-\phi)}{(3p-q)^2}\right\}$$
$$+\gamma\frac{e f^3}{2}\left\{\frac{\cos(\theta+3\phi)}{(p+3q)^2}+\frac{\cos(\theta-3\phi)}{(p-3q)^2}\right\};$$

so that there are six summation-tones and six difference-tones produced by direct transformation of the primaries, when the effect of terms up to the fourth order is considered.

70. The effect of the neglect of n^2 in the denominators of all these terms, is to place the principal development of any term such as the difference-tone $p-q$ at the point where $p-q=0$, whereas if the complete solution were retained the condition for the principal development would be

$$n^2-(p-q)^2=0.$$

No known phenomenon enables us to distinguish between these two cases. Every thing happens, so far as we know, precisely as if the simpler condition were that which is really important.

71. If we proceed to terms of higher orders in the same way, we shall always have, in the result of terms of the $\overline{n+1}$th order, the two following terms representing nth difference-tones, which alone are important for our present purpose (α_{n+1} is the $\overline{n+1}$th coefficient),

$$\frac{(n+1)\alpha^{n+1}}{2^n}\left\{\frac{e^n f\cos(n\theta-\phi)}{(np-q)^2}+\frac{e f^n\cos(\theta-n\phi)}{(p-nq)^2}\right\},$$

besides other terms analogous to those shown above.

72. In the neighbourhood of any consonance of the form $h:1$, the terms having the denominators $(hp-q)^2$ become large; this is Helmholtz's explanation of the origin of difference-tones, generalized.

73. As the argument from the analytical expressions fails to give perfect satisfaction unless the nature of the causes

involved be more directly demonstrated, I shall try to show more simply how it is that this comes about.

In periodic functions such as $\cos pt$, $\cos(qt-\epsilon)$, the quantities p, q are such that, if τ, τ' be the periodic times,

$$p\tau=q\tau'=2\pi, \text{ or } p=\frac{2\pi}{\tau}, \quad q=\frac{2\pi}{\tau'}.$$

If, then,

$$M\tau=1,$$
$$N\tau'=1,$$

M, N are the frequencies of the primaries, and

$$p=2\pi M, \quad q=2\pi N.$$

In the case of a mistuned consonance of the form $h:1$, the denominator of the h difference-tone term in the above expression will be $4\pi^2(hM-N)^2$. And $hM-N$ is the frequency of the beat which gives rise to the transformation according to all theories (putting $k=1$ in the more general formula $hM-kN$).

$\therefore \dfrac{1}{hM-N}$ is the time of duration of the beat of the resultant form, whether we call it the Smith's beat, or the bow of the pendulum curves. As the denominator diminishes, the time or duration of the beat increases.

74. What happens, then, is that a force is developed, by the influence of the higher terms in question, which acts for a time corresponding to the duration of the beat. And it is matter of ordinary mechanical knowledge that, under these circumstances, the space traversed is proportional to the square of the time during which the action lasts; so that when the beat is lengthened the effect of the transformation is strengthened.

75. It is possible to found an independent treatment of the subject on these considerations, the course of which would be somewhat as follows.

In mistuned consonances of the form $h:1$ there are alternate increases and diminutions of the maximum resultant displacement, the duration of which can be arrived at by the considerations employed by Smith in determining the duration of beats. The duration of one such increase and diminution can be shown by the known formula to be $\dfrac{1}{hM-N}$.

Assume that the transmitting mechanism of the ear possesses such powers of transformation that any regular sequence of increases and diminutions of maximum resultant displacement is capable of giving rise, by transformation, to a subjective note having the same period as that of one increase and diminution. This assumption only differs from that made above in definiteness of form; for the algebraic series which is above proved to give rise to transformations of this description, is itself an assumption.

It immediately follows, by considerations differing little rom those made use of in the ordinary investigation of the motion of a particle under the action of a uniform force, that the coefficient of the term in question will contain the square of the periodic time—that is to say, the coefficient $\frac{1}{(h\mathrm{M}-\mathrm{N})^2}$; and this is the essential point proved by the more complete analytical investigation above given.

76. Though perhaps defective as a complete demonstration of the *rationale* of the origin of difference-tones, these considerations render the general meaning of the coefficients of the difference-tone terms in the above equations tolerably clear. And we have thus sketched a method, in which the doctrine of transformation arising out of the Smith's beats, as the resultant forms pass through the transmitting mechanism of the ear, forms the basis for the further explanation of the phenomena of beats as we find them.

The Resultant Wave-forms of Mistuned Consonances.

77. I am principally acquainted with these forms as drawn by means of Donkin's harmonograph. The curves (Plates IV.–VII.) that accompany this paper exhibit all the points on which it will be necessary to touch.

78. It is hardly possible to be acquainted with these curves without seeing that the figures formed by the vertices which occur in the curves are in some way related to the phenomena of the mistuned consonances. And as I had myself considerable difficulty in coming to definite conclusions as to the real nature of this relation, and do not know of any published discussion of the subject, I add this article dealing with the relation in question so far as it is connected with the subject of the paper.

79. The curves are referred to an axis of x, along which the wave-lengths are measured, and an axis of y parallel to which the displacements are measured. λ and λ' are the wave-lengths on the paper of the two primary curves. If it is required to consider a question of frequency, the paper must be supposed to be drawn past the observer with velocity v, when the frequencies will be $\frac{v}{\lambda}, \frac{v}{\lambda'}$ respectively.

80. The tangent of the inclination of a curve to the x-axis will be spoken of shortly as the "slope."

It is assumed that $q\lambda' = p\lambda + \delta$, where p, q are integers, $q > p$, and δ is small.

81. The equation of the resultant of two primary curves may then be written

$$y = \mathrm{E} \cos 2\pi \frac{x}{\lambda} + \mathrm{F} \cos \frac{2\pi}{\lambda'}(x - \alpha).$$

The slopes of the two single curves are

$$-\frac{2\pi}{\lambda} \mathrm{E} \sin 2\pi \frac{x}{\lambda}, \quad -\frac{2\pi}{\lambda'} \mathrm{F} \sin \frac{2\pi}{\lambda'}(x - \alpha).$$

The ratio of the coefficients is

$$\frac{\lambda' \mathrm{E}}{\lambda \mathrm{F}} = \frac{p\mathrm{E}}{q\mathrm{F}} \text{ nearly.}$$

When this ratio is much greater than unity, the resultant slope is nearly that of the first term. When it is much less than unity, the resultant slope is nearly that of the last term.

82. The general expression for the resultant slope is given by

$$\frac{dy}{dx} = -\frac{2\pi}{\lambda} \mathrm{E} \sin 2\pi \frac{x}{\lambda} - \frac{2\pi}{\lambda'} \mathrm{F} \sin \frac{2\pi}{\lambda'}(x - \alpha).$$

The vertices of the resultant curve are obtained by equating $\frac{dy}{dx}$ to zero, whence

$$\frac{\mathrm{E}}{\lambda} \sin \frac{2\pi x}{\lambda} + \frac{\mathrm{F}}{\lambda'} \sin \frac{2\pi}{\lambda'}(x - \alpha) = 0.$$

83. Case I., where F is great, and the first term negligible compared with the second ($\frac{\lambda' \mathrm{E}}{\lambda \mathrm{F}}$ small).

Here the vertices are those of the second component of the curve. Consequently, in every cycle of p and q vibrations of

λ and λ' respectively, the q vertices of λ' appear, those of λ being smoothed out. The sole effect of the term involving λ is in this case to modify slightly the positions of the vertices.

84. If, then, $p\lambda$ were exactly $=q\lambda'$, then after a certain distance, which may be called a short cycle, the vertices would recur for precisely the same values of y. And the corresponding vertices in successive short cycles would lie on q straight lines, or on $2q$ straight lines if the lower vertices be included.

This short cycle is obviously $p\lambda=q\lambda'$ in duration.

85. Since, however, in our general case $p\lambda+\delta=q\lambda'$, the coincidence after the short cycle is not exact; but the vertex determined by equating the second term of the inclination to zero has, in the first term, a different correction to the value of y from that which existed before the short cycle.

86. At the vertex before the short cycle let $x=\alpha$, so that the second term of the inclination vanishes; then, before the short cycle,

$$y_0=\mathrm{E}\cos\frac{2\pi\alpha}{\lambda}+\mathrm{F};$$

after one short cycle, $x=q\lambda'+\alpha=p\lambda+\delta+\alpha$,

$$y_1=\mathrm{E}\cos\frac{2\pi}{\lambda}(\alpha+\delta)+\mathrm{F};$$

after two short cycles, $w=2q\lambda'+\alpha=2(p\lambda+\delta)+\alpha$,

$$y_2=\mathrm{E}\cos\frac{2\pi}{\lambda}(\alpha+2\delta)+\mathrm{F};$$

and so on, till after n short cycles, where $n\delta=\lambda$, nearly or exactly,

$$y_n=\mathrm{E}\cos\frac{2\pi\alpha}{\lambda}+\mathrm{F};$$

and the ordinate of the vertex in question has gone through a complete period of a pendulum-curve in the space

$$\begin{aligned} nq\lambda' &= n(p\lambda+\delta) \\ &= n\lambda\left(p+\frac{1}{n}\right), \text{ since } n\delta=\lambda \\ &= \lambda(np+1). \end{aligned}$$

87. Now we have seen that there are q of these vertices, each of which gives rise to one of these curves. Consequently

this space, $(np+1)\lambda = nq\lambda'$, presents, both above and below, q projecting bows, and each bow is of length

$$\frac{np+1}{q}\lambda \text{ or } n\lambda'.$$

This is the length of Smith's beat, or of the beat as given by a well-known formula. This is easily verified as follows:—

88. Let v be the velocity of sound corresponding to wave-lengths λ and λ', and M, N the corresponding frequencies; then

$$\frac{v}{\lambda} = \mathrm{M}, \quad \frac{v}{\lambda'} = \mathrm{N},$$

and $q\lambda' = p\lambda + \delta$ becomes $(n\delta = \lambda)$,

$$\frac{q}{\mathrm{N}} = \frac{p}{\mathrm{M}} + \frac{1}{n\mathrm{M}};$$

$$\therefore\ p\mathrm{N} - q\mathrm{M} = \frac{\mathrm{N}}{n} = \frac{v}{n\lambda'};$$

which connects the expression above obtained with the ordinary formula for the frequency of the beat. Hence the Smith's beat in this case corresponds in period to the projecting bow formed by the $\frac{1}{q}$th part of the whole periodic curve of slow disturbance of one of the vertices.

89. Case II., where $\frac{\mathrm{E}}{\lambda} = \frac{\mathrm{F}}{\lambda'}$, so that the condition for a vertex reduces to

$$\sin\frac{2\pi x}{\lambda} + \sin\frac{2\pi}{\lambda'}(x-\alpha) = 0.$$

This condition gives the following series of values:—

$$\begin{aligned} \frac{x}{\lambda} &= \frac{-x+\alpha}{\lambda'}, & x &= \frac{\alpha\lambda}{\lambda+\lambda'}, \\ &= \frac{-x+\lambda'+\alpha}{\lambda'}, & &= \frac{(\alpha+\lambda')\lambda}{\lambda+\lambda'}, \\ &\cdots\cdots & &\cdots\cdots \\ &= \frac{-x+\nu\lambda'+\alpha}{\lambda'}, & &= \frac{(\alpha+\nu\lambda')\lambda}{\lambda+\lambda'}, \end{aligned}$$

until

$$\nu\lambda' = \lambda + \lambda';$$

and if

$$q\lambda' = p\lambda,$$

$$\nu = \frac{p+q}{p}.$$

Where this is not a whole number, the condition will be

$$\nu\lambda' = k(\lambda+\lambda'),$$

$$\nu = k\,.\frac{p+q}{p};$$

and if $p : q$ is in its lowest terms,

$$k=p, \text{ and } \nu = p+q,$$

$p+q$ is therefore the number of independent vertices arising from these terms.

90. Another series of values satisfies the condition; these are as follows (since $\sin x = \sin(\pi - x)$ &c.):—

$$\frac{x}{\lambda} = \frac{1}{2} + \frac{x-\alpha}{\lambda'}, \qquad x = \frac{\lambda'-2\alpha}{2(\lambda'-\lambda)}\,.\lambda,$$

$$= \frac{3}{2} + \frac{x-\alpha}{\lambda'}, \qquad = \frac{3\lambda'-2\alpha}{2(\lambda'-\lambda)}\,.\lambda,$$

$$\cdots\cdots \qquad \cdots\cdots$$

$$= \frac{2\nu-1}{2} + \frac{x-\alpha}{\lambda'}, \qquad = \frac{(2\nu-1)\lambda'-2\alpha}{2(\lambda'-\lambda)}\,.\lambda,$$

until

$$(2\nu)\lambda' = 2(\lambda-\lambda'),$$

$$\nu = \frac{\lambda'-\lambda}{\lambda'},$$

$$= \frac{p-q}{p}.$$

And if this be not a whole number,

$$(2\nu)\lambda' = 2k(\lambda-\lambda'),$$

$$\nu = k\left(\frac{q}{p}-1\right),$$

$$\nu = \frac{k}{p}(p-q);$$

and if $p : q$ is in its lowest terms,

$$k=p, \quad \nu = p-q, \text{ or } q-p, \text{ since } q>p;$$

$q-p$ is therefore the number of independent vertices arising from these terms.

91. The relation of these different sets of vertices may be otherwise exhibited by putting the expression for the inclination into the form

$$\sin\frac{2\pi x}{\lambda}+\sin\frac{2\pi}{\lambda'}(x-\alpha)$$
$$=2\sin\pi\left\{x\left(\frac{1}{\lambda}+\frac{1}{\lambda'}\right)-\frac{\alpha}{\lambda'}\right\}\cos\pi\left\{x\left(\frac{1}{\lambda}-\frac{1}{\lambda'}\right)+\frac{\alpha}{\lambda'}\right\}=0.$$

The zero values of the sine give the $p+q$ vertices of the first set; and the values which make the cosine vanish give the $q-p$ vertices of the other set.

92. Each of these vertices occupies, as in the former case, its special position in the short cycle $q\lambda'=p\lambda$, and lies always on a straight line $y=$ constant when such an exact relation holds.

93. Also, as before, when the above relation is changed into $q\lambda'=p\lambda+\delta$, it may be shown, by examining the successive arguments of the vertices, that they shift their position in successive short cycles, so that they lie on pendulum-curves of long period; also that the period of these curves is, for the $p+q$ system, $p+q$ times that of the Smith's beat, and for the $p-q$ system, $p-q$ times that of the Smith's beat.

94. The curves of both these systems, with the Smith's beats which form part of them, are readily recognized in all those of the pendulum-curve illustrations which approximately satisfy the condition

$$p\mathrm{E}=q\mathrm{F} \text{ or } \lambda'\mathrm{E}=\lambda\mathrm{F}.$$

In the case of the major third, where there are many vertices in each short cycle, and the figure of the short cycle is itself complicated, these curves are not easily recognized. It is necessary to mark a set of corresponding vertices in order to recognize the curve in this case. By the time we arrive at the fifth the curves are quite plain.

The curves of the $p+q$ system are large and bold, extending completely from top to bottom of the illustrations; each curve comprises the bow of a Smith's beat both above and below. These may be spoken of as the external system.

The curves of the $q-p$ system are smaller, and lie nearer the axial line of the illustrations. These may be spoken of as the internal system. In the particular case where $q-p=1$,

such as $q=2$, $p=1$, the internal system exhibits complete periodic curves having the period of the Smith's beat.

95. Case III., where F is so small that qF is small compared with pE. This would fall under the argument of the first case, with the signification of the letters reversed. But as we made the convention $q>p$, there arise some special points of difference.

96. Where q is much greater than p, as is the case of high harmonics combined with a fundamental, F has to be very small indeed in order that qF may be small compared with pE. In this case, unless F is almost evanescent, it is not generally true that the only vertices are those of E (the fundamental); for in these cases the vertex of the fundamental curve becomes almost a straight line in the short space occupied by a wave of the higher curve; and under these circumstances the vertices of the higher curve continue to be visible wherever they come upon the vertex of the lower, especially where the two vertices are turned opposite ways.

The conditions of case I. not being strictly fulfilled, the consequences there deduced do not strictly follow. The considerations as to the number of different vertices which develop curves are not materially affected. And it remains true that there are always p curves (in case I. q curves) actually developed; but it is not true that there are no traces of any of the other q curves of the entire external set of $p+q$ of case II. On the contrary, it is seen in several of the illustrations, where for the most part $p=1$, that, instead of the outline being one pendulum-curve embracing the outlines of all the Smith's beats, the internal vertices of the long curves present traces of the crossing of two pendulum-curves of longer period—an effect which is seen to survive from the more general cases, on comparing the illustrations to case II. As the amplitude of the higher note diminishes, this curve assumes a trochoidal form, the external vertices being less sharp than the internal, where there is the survival from the crossing. Ultimately, no doubt, the outline would become theoretically a pendulum-curve. But, in the case of indefinite diminution of the coefficient $\frac{q\text{F}}{p\text{E}}$, where $\frac{q}{p}$ is great, $\frac{\text{F}}{\text{E}}$ is of the order of the product of two small quantities; consequently the effects on the displacements, or

the curves we are examining, would themselves tend to become evanescent before their peculiarities; consequently the curve enveloping the Smith's beats would never in this way be reduced to a pendulum-curve having the period of those beats.

In the application of these considerations we have, further, to remember that the resultant tones which present pendulum-curves having the periods of Smith's beats are only heard when both notes are pretty loud; and under these circumstances the indefinite diminution of the ratio above supposed is not admissible. The only case, therefore, in which a locus of vertices is a pendulum-curve of the same complete period as the period of Smith's beat, is that of an internal system under case II., where $q-p=1$. As the existence of this system depends on the accurate adjustment of the coefficients to the law $p\mathrm{E}=q\mathrm{F}$, it cannot be referred to even as an illustration of a phenomenon of general occurrence.

97. We conclude, in conformity with the explanation at the end of the former part, (1) that the forms exhibited by the resultant of two pendulum-curves do not, as a rule, exhibit any appearances corresponding to pendulum-curves having the period of the Smith's beat, except in a very small number of cases, the conditions for which can only be fulfilled by accident; (2) that the increases and diminutions of the maximum displacement which form what we have called the bows of the harmonic curves, correspond in duration with the Smith's beat, but not in the period of the harmonic curves of which they form part.

98. We infer from the previous parts of this paper:—(3) that the variations of maximum displacement which are represented in these figures by the bows of the harmonic curves, give rise by transformation to pendulum-vibrations having the same frequency as those variations—these being the notes which König calls beat-notes, and Helmholtz difference-tones of various orders; and (4) that the actual beats of mistuned consonances of the form $h : 1$, as heard by the ear, are given rise to by the interference of these beat-notes or difference-tones with the lower note of the combination.

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99. The upper numbers prefixed to the Plates of curves are

the ratios of the wave-lengths; the lower ones the ratios of the amplitudes.

Typical curve of case I. .	$\frac{27}{80}$	
	9 : 8	
Typical curve of case II. .	$\frac{27}{80}$	
	3 : 1	
Typical curve of case III.	$\frac{27}{80}$, or rather	$\frac{1}{5} \times \frac{80}{79}$
	9 : 2	10 : 1

POSTSCRIPT.—The curves shown in the Plates are all illustrations of the subject of this paper, with the exception of three sets; namely, the combinations of vibrations whose wave-lengths are nearly as 4 : 5, as 2 : 3, and as 2 : 5. These have been given for the sake of completeness in the collection of curves, and that readers may have the opportunity of seeing the nature of the difference between such curves as these, which may be said to belong to mistuned consonances of the form $h : k$, and our normal forms which belong to mistuned consonances of the form $h : 1$.

XXV. *On the Transmission of Radiation of Low Refrangibility through Ebonite. By* Capt. ABNEY, *R.E., F.R.S., and* Col. FESTING, *R.E.**

WHEN Mr. Graham Bell described his interesting experiments with the photophone, we were much surprised to learn that an effect was produced when sheets of ebonite of small thickness were interposed between the apparatus and the source of radiation; and it became a matter of more than curiosity to us to know what was the cause of the phenomenon, since photographic manufacturers were commencing to use ebonite in the construction of the dark slides for the camera.

We think we can demonstrate, however, that the ordinary explanation of transmission of radiant energy can account for the phenomenon. Dr. Guthrie kindly furnished us with a sheet of ebonite, through which the action of a beam of

* Read April 9, 1881.

radiation on a selenium cell was most marked; and we accordingly first experimented with that. A photographic spectroscopic apparatus was employed, of the form we have already described in other papers; and the compound of silver was used which is sensitive to all parts of the spectrum. At first we employed only one prism, and used the sun as a source of illumination; and here it may be parenthetically remarked that on the evening when we made our first experiments the wind was blowing from the north-east, and there was a clear sky. Half the slit was covered up, a piece of ebonite placed in front of the other half, and a plate exposed to the action of the spectrum of the radiations (if any) coming through this thin layer of apparently opaque matter. An exposure of three minutes was given; the exposed half of the slit was then closed and the other half opened, and a spectrum taken through a solution of bichromate of potash $\frac{1}{10}$ inch thickness. This bichromate was used to prevent the too energetic action of the more refrangible rays, which illuminated the prism and would have caused a veil over the plate. Half a minute's exposure was given. The plate, on development, revealed that rays of very low refrangibility had passed through the ebonite, commencing at W.L. 12,000 and extending as far as W.L. 7500; the point of maximum intensity was situated at about 9000. The photographs were on a small scale, but sufficed to show the absorption of the ebonite. On the next day we had intended to repeat the experiments with two or three prisms; but the wind had shifted, and the solar spectrum was absorbed as far as about 9000, showing the presence of aqueous vapour. It was therefore useless to experiment further with the sun as a source of radiation; so we used the water of the positive pole of an electric light as a source. It will be seen that the spectrum through ebonite extends to about W.L. 15,000, and then terminates.

The next point to determine was as to the quality of the beam coming through the ebonite. This we determined as follows—first by placing a piece of ebonite in contact with the photographic plate and throwing an image of the points on it, and thus getting an impression, and then, by a simple arrangement, removing the ebonite to a distance of 1 foot, and allowing the beam to traverse it, and securing another image on a different plate. The photographs showed that the rays are

very much scattered in their passage through the ebonite, no distinct image being formed in the latter case, though it was sharp and defined in the former. The amount of scattering it seemed desirable to know. For this purpose the collimator of the spectroscope was used and no prism, the image of the slit $\frac{1}{20}$ inch wide was focused on the focusing-screen of a camera, and a piece of ebonite was placed in contact with the plate, and exposure made. This piece was removed and another piece inserted $2\frac{1}{2}$ inches in front of the plate, and another exposure given. The diffusion was most marked: a line $\frac{1}{20}$ inch broad was diffused over a space $\frac{1}{4}$ of an inch, most intense, of course, in the centre. By subsequent experiment it was shown that an exposure of three times the length of that given in the first case was necessary to cause the central portion of the band in the second case to correspond in intensity with that of the image of the slit in the first case. With two pieces of ebonite in contact with the plate six times the exposure was required to give the same intensity as with only one plate of ebonite intervening. Hence we may say that the coefficient of absorption of a plate of ebonite $\frac{1}{64}$ of an inch in thickness $=1{\cdot}8$; and a calculation will show that any rays which can penetrate through $\frac{1}{8}$ of an inch of ebonite will only have an intensity of $\frac{1}{1650000}$ that of the resultant beam, without deducting any thing for the scattering of light. In fact, with the electric light and a wide slit an hour's exposure produced no effect on the photographic plate when ebonite $\frac{1}{8}$ in. in thickness was placed before the slit. It must, however, be remembered that ebonite varies in quality; sometimes the outside alone is black, the inner portions resembling gutta percha in colour. With specimens of this sort a greater thickness could no doubt be traversed than $\frac{1}{8}$ inch. In such a case, however, we doubt if the substance would be true ebonite.

In a communication to 'Nature,' Messrs. Ayrton and Perry show how they determine the refractive index of ebonite by an arrangement with the telephone. They use a prism; and we should judge by the figure they give that the thickness of ebonite traversed must be about $\frac{1}{4}$ of an inch; so that the radiations transmitted must be very small. We may remark that the direction of a beam of light issuing from a prism formed of a turbid medium would not have its maximum in-

tensity in the true direction of refraction ; it would be slightly displaced. Mr. Preece, in a recent communication to the Royal Society, remarked that some ebonite he tried was as transparent as rock-salt; and so it is if a thin-enough layer be taken; and we think that it was the minute layer that was taken that caused this expression to be used. He also stated that another sample equally thin was perfectly opaque to radiation. Through his kindness we were able to experiment with the identical samples to which he refers. The "transparent" specimen behaved as that we have already described; the opaque one showed that the radiations were more scattered in their passage through it. We may state that, by examining the thin ebonite with which we first experimented, we could see a trace of the sun's image through the material, and very faintly through two layers. The radiations of low refrangibility were evidently more copiously passed, since when an image of the sun formed by a lens was caused to fall on a piece of paper and a sheet of thin ebonite interposed, if the eye or hand was placed at the focus considerable warmth was felt.

It became interesting to know whether the ebonite was merely a mechanical mixture of sulphur and india rubber or a chemical combination. Placing a piece of stout india rubber, about the same thickness as the ebonite, before the slit of the spectroscope, and with an exposure of ten minutes, no vestige of an image was found on development of the plate. This was evidently owing to the great scattering of the rays by the substance. The india rubber being laid in contact with the plate, and an exposure made through it, showed that it was transparent to all rays from 10,000 to 5000. The absorption-spectrum therefore differed; and it is evident that in ebonite the india rubber is chemically changed in composition.

The conclusion to be drawn is, that ebonite, when of small thickness, transmits to some extent the rays of low refrangibility.

XXVI. *Note on Stereoscopic Vision.* *By* Professor HELMHOLTZ*.

THE motives by which we judge the distance of the objects before us with one eye are the following :—

1. The outlines of the more distant objects are covered by those of the nearer, where they meet. It is this circumstance which causes the difficulty we have in recognizing the fact that the image projected by a convex lens or a concave mirror is nearer to the observer than the lens or the mirror.

2. The object which throws a projected shadow upon any surface is situated always before that surface. These first two motives are very rarely overpowered by any other ones —as, for instance, by stereoscopic combination. This is easily demonstrated by Dove's pseudoscope, an instrument containing two rectangular prisms, and showing to each eye a reflected image inverted from right to left. It produces an inverted stereoscopic effect if there are no projected shadows, and no outline covered by the outline of a nearer body.

3. If the head is moved to the right or the left, upwards or downwards, the direction of the eyes remains steady if the object observed is infinitely distant, but is altered the more the nearer the object is. If the head is brought nearer to the object, the convergence of the eyes increases ; if it is moved backwards, the convergence diminishes.

We may call the peculiarity of the eye producing these phenomena the *parallax for motion of the head.* At the same time the relative situation of objects of different distance in the field of vision is altered. Distant objects apparently go with the observer, nearer objects in opposite directions. Also this overpowers stereoscopic combination. In Brewster's stereoscope the two images are brought together with a moderate convergence of the eyes, but are nearly in the focus of the lenses, so that these project images at an infinite distance. The instrument is adapted to presbyopic vision. But if it is fastened to a table and the head of the observer is moved, the objects appear to be far more distant than the point of convergence.

I concluded from this and similar observations that the per-

* Read April 9, 1881.

ception of the absolute convergence of our eyes is very indistinct, and that only differences of convergence, related to apparently near or distant objects, produce the stereoscopic effect. But lately I have observed that certain apparent motions of binocular objects may be observed, which prove that the incongruence between the degree of convergence and the parallax of motion is perceived with great accuracy.

The easiest way to see them is the following :—Look to a papered wall, the pattern of which is regularly repeated at distances not much greater than the distance between your eyes (between 60 and 70 millim.). You know that it is possible to make your eyes converge either to a nearer or to a more distant point than the surface of the paper ; so that in your binocular field of vision two images get corresponding position, which do not belong to the same part of the paper, but to two different copies of the pattern. You see, then, a stereoscopic image of the pattern, either more distant and of greater apparent size, if you diverge your eyes, or nearer and smaller, if you converge. But the appreciation of the apparent distance of this pattern is not very precise. If you try to bring a pencil to the apparent place of the nearer pattern, you will find that the point of convergence is far nearer than the apparent place of the pattern.

When you now move your head the pattern moves also. If you have increased the convergence of the eyes, the pattern moves with the eyes, as well to the left and the right as up and down and forwards and backwards. If you diverge, it goes in opposite direction to your head.

XXVII. *Note on Thermal Electrolysis.*

By J. H. Gladstone, *F.R.S., and* Alfred Tribe.

During the course of our experiments on metallic replacements we noticed that some sheet silver, immersed in fused silver chloride, became quickly studded with crystals of metal. A replacement of a metal by itself seemed so anomalous, that our first idea was that the silver employed

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contained certain impurities; but we found that the action took place just as well with the purest silver we could obtain, and that it was not restricted to the substances above mentioned. Not only might the iodide of silver be substituted for the chloride with the same result, though not so rapidly effected, but other metals might be employed. Thus, when copper was immersed in fused cuprous chloride, crystals of that metal separated; and similar exchanges took place when zinc was placed in melted zinc chloride, or iron in ferrous chloride in a molten condition.

It was then thought that a different physical condition of the rolled metals might give rise to the action; but this was disproved by the following experiments:—

Some crystals of silver prepared by electrolysis were placed in the open end of a piece of glass tubing slightly constricted, and then immersed in silver chloride heated in a crucible by a Bunsen lamp. In about half an hour the crystals were found to have grown in a net-like mass from their original position to a point about half an inch higher in the tube. This experiment was repeated with crystals of silver which had themselves been deposited from the fused chloride by means of metallic silver. A similar result was obtained.

We were then led to the conclusion that the change depended upon the unequal heating of different parts of the immersed metal, or rather of the salt in which it was immersed. It is evident that upon the contact theory of voltaic action, there will be a difference of potential between the metal and the liquid chloride with which it is in contact; and it is in accordance with analogy to suppose that this difference of potential will vary according to the temperature. Now, under the conditions of the experiment, it cannot be supposed that all parts of the fused chloride in contact with the immersed metal were always equally heated; and we have therefore the possibility of a current being established with the consequent electrolysis of the salt.

In order to test this view, some silver chloride was fused in a hard glass tube and a rod of silver placed in the liquid. On heating the underside of the lower end for 10 minutes, we found a considerable crop of silver crystals in the comparatively cool part of the fluid.

In another experiment some silver chloride was fused in a crucible, and one side of the vessel was more strongly heated than the other. Two long rods of silver were connected with a galvanometer and placed, one in the hotter, the other in the colder part of the chloride. The latter was found studded with crystals at the end of 15 minutes, whilst the former was quite clean. On repeating this experiment, it was always found that the galvanometer gave a larger deflection the greater the difference of temperature between the portions of the fused mass penetrated by the silver wires, and that the current was reversed with a reversal of the rods. Copper wires in cuprous chloride gave similar results.

In an experiment with an electrometer we obtained a clear indication of a difference of potential between silver rods in hotter and colder parts of silver chloride fused in a small crucible, the deflection showing a difference of possibly $\frac{1}{50}$ of a volt. The reversal of the rods again produced a reversal of the deflection.

In corroboration of the theory above stated, it should be borne in mind that the chlorides of silver, copper, zinc, and iron, when fused, are electrolytes. The liquid chloride of tin is not an electrolyte; and it was found that on immersing tin in this liquid no deposition of crystals was observed when it was so arranged that one part of the liquid was kept at the heat of boiling water and another at the ordinary temperature for two days; nor was there the least action on a galvanometer when arrangements were made for testing by that instrument.

These experiments form a good lecture-table illustration of the conversion of heat into electricity and chemical force. They also seem to have a bearing on the theory of voltaic action, since, from the nature of the substances employed, it is difficult to imagine that chemical action in any way initiates the current.